DEAR RUSSELL - DEAR JOURDAIN

I. Grattan-Guinness

Dear Russell—Dear Jourdain

A commentary on Russell's logic, based on his correspondence with Philip Jourdain

I. Grattan-Guinness

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Prologue

1. *Biographical introductions*

In order to appreciate the correspondence between Bertrand Russell and Philip Jourdain, it would be useful first to describe the backgrounds of the correspondents. Russell's life has already been related very well (especially by himself),[1] and so we need recall only briefly the course of his early years. His parents died soon after his birth in 1872, and he was brought up in the home of his grandparents on his father's side. From an early age he showed a profound natural interest in both mathematics and philosophy, and going up to Trinity College, Cambridge in 1890 as a minor scholar he took the Mathematics Tripos for Part I in 1893 and the Moral Sciences Tripos for Part II in the following year.

From the beginning of his studies Russell saw mathematics as the source of exact and certain knowledge. 'I hoped sooner or later to arrive at a perfected mathematics which would leave no room for doubts', he recalled later, 'and bit by bit to extend the sphere of certainty from mathematics to other sciences' (*1959a*, 36; compare *1956a*, 53). But the experience of mathematics at Cambridge was a great disappointment to him. He said little about it in his later writings, apart from stressing its badness (*1959a*, 37–38); but fortunately for posterity his contemporary Grace Chisholm, who was in the year senior to his at Cambridge, was much more expansive in her reminiscences:

> Mathematical science had reached the acme of perfection. Through the long future ages, no new ideas, no new methods, no new subjects were to appear. The edifice of mathematical science was complete, roof on and everything. All that remained to be done was to consolidate and repair the masonry, and add to and correct the ornamentation . . .
>
> At Cambridge the pursuit of pure learning was impossible. There was no mathematician—or more properly no mathematical thinker—in the place. The depressing character of the intellectual

[1] See especially Russell *1967a*, though *1944a*, *1956a* and *1959a* also provide details. (The method of citation of literature is described at the beginning of the bibliography on pp. 185–186 below.) See also Clark *1975a*.

> atmosphere was due to the examination. Everything pointed to examinations, everything was judged by examination standards, progress stopped at the Tripos . . .[1]

After graduation, Miss Chisholm studied with Felix Klein at Göttingen and became the second woman to obtain an official doctorate in Germany.[2] Russell also exiled himself from Cambridge to some extent, for after his brief career in the British embassy in Paris and his marriage to Alys Pearsall Smith in 1894, he lived mostly away from Cambridge. In 1895 he obtained a Fellowship at Trinity for his essay on the foundations of geometry (published as *1897b*), and then started to write sequel volumes on the foundations of physics and of arithmetic.[3] But he was dissatisfied with these efforts, and did not find a new orientation for his work until the International Congress of Philosophy at Paris in August, 1900, which he attended with his friend and former teacher A. N. Whitehead. There he discovered the work of Peano, and was able to restructure and extend his previous work along Peanesque principles. It is at this point that his correspondence with Jourdain will take over, and provide us with new information not only on the later developments but also on the earlier work whose course has just been summarised. Plate 1 shows him at this threshold stage of his career.

Philip Jourdain was born in 1879, seven years after Russell.[4] He was the youngest of five sons and ninth in a family of ten children. His father was the vicar at Ashbourne, Derbyshire. Several of the children showed in later life interests in problems that amounted to obsessions. Of Philip's we shall hear a great deal, so for now we may briefly note the careers of some of the other children. The eldest daughter Eleanor became Vice Principal of St. Hugh's Hall (as it was then called), Oxford. She is best remembered for the book that she published pseudonymously in 1911 with her Principal Annie Moberly (whom she eventually succeeded for a short while), in which they claimed that during a visit to Versailles in 1901, and also during a visit by Eleanor alone the following year, they saw Marie Antoinette and members

[1] See my *1972b*, 115 and 131. These remarks are unfair to Whitehead, with whom Miss Chisholm seems to have had little or no acquaintance. Valuable insights on Whitehead's conception of mathematics are provided in Lowe *1975a*.

[2] My *1972b* gives the story of her marriage with W. H. Young, whom she married in 1897. Sophie Kowalewskaya received a doctorate in her absence from Göttingen in 1874.

[3] In the Russell Archives see especially the manuscripts 'The fundamental ideas and axioms of mathematics' (1898) and 'Principles of mathematics' (1899–1900). (On the Russell Archives, see p. 5 below.)

[4] The principal obituaries for Jourdain are Heath *1920a*, Loria *1921a* and Sarton *1923a*; see also Wrinch *1921a*. I am also indebted for additional information from J. M. Rollett.

of her court.[1] Another daughter, Margaret, is still known today for her various books on furniture, though she also prepared translations of others' work and wrote on social questions. She was a life-long friend and companion of Ivy Compton-Burnett. The eldest son Francis, who was named after his father and also became a vicar, was passionately interested in birds and became President of the British Ornithological Society; a 'Jourdain Society' exists to this day. He was the only one of the children who himself had children. Two other brothers, Charles and Henry, became Lieutenants-colonel in the British army, and Henry, who published many pamphlets about his regiment, suffered the last mutiny that has occurred in the British army to date.

But the member of the family who was closest in spirit to Philip was his youngest sister Millicent. She published pseudonymously in 1913 some reminiscences of her childhood, in which Philip appeared prominently under his third Christian name 'Bertrand'.[2] The closeness of her attachment to Philip was probably due to the fact that they both suffered from a creeping paralysis known as 'Friedreich's ataxia'. Russell described the illness as it affected Philip in an account written in his personal journal of a visit that he paid to Philip and his family in November, 1902:

> He is very ill, partially paralysed, and at first sight almost half-witted. But as soon as he begins to talk of mathematics his face shines, his eyes sparkle, he speaks with fire and ability, one forgets that he is ill, or remembers it only in passionate admiration of the triumph of mind . . . I saw his mother for a moment: she pressed my hand, I loved her, and she seemed deeply grateful to me for encouraging her son: it was a deep moment of intimacy, though not a single word was said by either of us, and I had never met her before.[3]

After schooling at Cheltenham College, Jourdain went up in 1899 with a scholarship to Trinity College, Cambridge. His father had died by then, and so his mother moved from Ashbourne to Cambridge with

[1] The book was Morison and Lamont *1911a*. For a study of this affair (in which, surprisingly, Philip is not mentioned) see Iremonger *1957a*.

Miss Moberly provides some interesting mathematical connections through her grandfather Edward, a merchant in St. Petersburg. Either he or his father was very friendly with Cantor's father (see Cantor's exchange with Jourdain on this point in my *1971a*, 123–124), while his wife was the aunt of A. Cayley (see Forsyth *1895a*, i).

[2] Arden *1913a*, with a preface by Russell's friend Gilbert Murray. According to this book, three other children were called Judith, Jill and Dick. Millicent drew the portrait of Philip in Sarton's obituary *1923a*. The other daughter was called Charlotte.

[3] Russell kept this journal from 1902 to 1905, and later gave it to Ottoline Morrell. It is now kept with her papers at the Humanities Research Center, University of Texas at Austin, U.S.A. This extract is quoted from a transcription made by Miss K. Timusk and now kept in the Russell Archives.

Millicent (and perhaps one or two of the other children) so that Philip could live at home while he attended the university. (Plate 4 shows him at this time in his life.) Even then, he missed his first academic year because of illness, and found later that, though he had taken his B.A. in mathematics in 1902, he was not eligible to apply for a Fellowship at Trinity. He also obtained only an honourable mention in 1904 for the Smith's prize (an annual mathematics competition at Cambridge). But in 1906 he won the Allen Studentship (as it was then called), a research scholarship then awarded biennially, for which Russell was one of his supporters.

Some time after his graduation Jourdain moved with his family to Yateley, Hampshire. Then from 1904 to 1911 he lived at Beaminster, Dorset, and from 1911 until 1915 at Girton near Cambridge. He married in 1914, and moved in the following year to Fleet, Hampshire, where he died of the increasing effects of his illness in 1919. Throughout all these years he lived on monies such as the Allen Studentship and some family resources, and otherwise on small remunerations from his many writings. He published his research work in various mathematical journals, while his historical and philosophical studies appeared in some of the same journals and also in others such as *The Monist* and *Mind*. In later years he did a vast amount of abstracting, especially for *The Monist*, *Isis*, *Jahrbuch über die Fortschritte der Mathematik* and *Science Progress*. For the Open Court Publishing Company, publishers of *The Monist*, he prepared a number of editions and translations of others' work. Similarly he edited several of the volumes in the *Ostwald's Klassiker* series. A frequent book reviewer, he also wrote articles for encyclopaedias. Mathematics was not his only interest, for he was a gifted writer of short stories and satirical verse, which during certain periods of his life appeared regularly in magazines such as the Cambridge University humorous journal *The Granta*. There is no comprehensive bibliography of his voluminous writings; the best list available is in Loria *1921a*, 181–184.

Thus Russell and Jourdain had much in common; not only their interest in the foundations of mathematics, but also the characteristics of industry, determination and strong self-will. Having surveyed each of them briefly in turn, we may now describe the circumstances which brought them together and led to their extensive correspondence.

2. *The nature of the surviving correspondence*

While still a student Jourdain attended Russell's lectures on mathematical logic in the winter of 1901–1902, and soon afterwards he began their correspondence. He was beginning to develop his research interests,

which lay mainly in Cantorian set theory, the foundations of mathematical analysis, and the principles of mechanics. At the same time he was starting to prepare historical studies in these branches of mathematics. Over the years he acquired a remarkably detailed and penetrating knowledge of both current research problems in his fields of interest and the historical situations from which they had arisen.

Such a union of technical and historical interests was less infrequent then than it is now. Indeed, the turn of the century was a bright period in research into the history of mathematics, with many distinguished mathematicians contributing major historical studies to *Bibliotheca Mathematica* and other journals, to the *Encyclopädie der mathematischen Wissenschaften*, and in the form of books. This kind of activity was especially evident in Germany, where men such as H. Burkhardt, F. Engel, A. Pringsheim and P. Stäckel stand out among many others. But Jourdain was close to their standards in his own fields, and was the most important British contributor to historical work during the period.

The nature and quality of Jourdain's interests are of great relevance to his correspondence with Russell, for in his letters he asked penetrating questions from both the technical and the historical points of view. Fortunately for posterity, Russell was quite ready to reply at considerable length. Their correspondence covered many aspects of logicism, with especially substantial discussions of theories of irrationals and the real line, the construction of transfinite ordinals and cardinals, possible solutions to Russell's and Burali-Forti's paradoxes, the possible provability and the ramifications of axioms of choice, and some of the theories which Russell attempted before his commitment to the type theory of *Principia Mathematica*. There are also extensive accounts by Russell of his discovery of the writings of influential predecessors. The only part of logicism which is neglected is the foundations of geometry, in which Jourdain's interest was slight and Russell's had become muted. In addition, a number of other matters are only lightly discussed, so that this volume is not to be taken as a comprehensive treatment of Russell's logic and its background.

It seems certain that Russell did not correspond with anyone else about his work in such great detail, for the letters that survive among his papers in the Bertrand Russell Archives at McMaster University in Hamilton, Canada do not contain any other collection of exchanges of this type.[1] Even the correspondence with A. N. Whitehead is not so historically interesting, for it tends to deal with matters of detail arising

[1] For a survey of the logical manuscripts and correspondence in the Russell Archives, see Blackwell *1969a*, and my *1974a*, 387–389. The Archives publishes an excellent quarterly called '*Russell*'.

As this book goes through the press, I hear that about 200 letters between Russell and Couturat have been discovered in the Bibliotheque de la Ville, La Chaux-de-Fonds, Switzerland.

from decisions of principle taken during personal conversation.[1] One of the reasons for the paucity of material is the fact that until his last twenty years Russell rarely received regular assistance in the organisation of his papers, and apparently did not even realise their possible value to posterity. The organisation of his papers was restricted to his occasional session of sorting or disposal of materials, with the result that a vast number of them were thrown away. The correspondence with Jourdain is a good example. In the Russell Archives there is barely a score of letters from Jourdain and only a few of Russell's replies, and of these many are after 1916. But the *total* preserved correspondence comprises 59 letters or carbon copies for Russell, and 57 letters or drafts (some of which are only summary drafts) of lost letters for Jourdain, together with a few related notes. Most of it comes, therefore, from Jourdain's side.

When Jourdain died in 1919 he was the English editor of *The Monist*. His widow appointed as literary executors A. E. Heath and J. M. Child, who were respectively the new English editor and the reviews editor for that journal. At the time the Swedish mathematician G. Mittag-Leffler was collecting material for proposed biographies of important mathematicians of the recent past, especially G. Cantor and K. Weierstrass. His house at Djursholm near Stockholm in Sweden is now run as the Institut Mittag-Leffler, a mathematical research institute, and contains several valuable collections of manuscripts which I have described in my *1971c*. Among Mittag-Leffler's own correspondence is a file of letters from Jourdain, which show that Jourdain had mentioned his correspondence with Cantor and in June 1919 was arranging to have them typed out for Mittag-Leffler's benefit. But his death a few months later stopped his plan (and also his intention to translate into English Mittag-Leffler's essay *1920a* on Weierstrass's theory of real numbers). However, in 1922 Mittag-Leffler asked Heath and Child if they would send him the Cantor letters. In May he received from Child two thick notebooks containing not only those letters but also many others. In addition, he was sent an unpublished paper by Jourdain on the provability of the well-ordering principle for judgement of its mathematical merit, and he published it in his journal *Acta Mathematica* as Jourdain *1922a*.

This ' generosity ' on the part of Heath and Child was probably a manifestation of the abandonment of their responsibilities to Jourdain's papers, for they never fulfilled their intention (recorded in Sarton *1923a*, 132, 136) to prepare an extensive biography and publish his unfinished

[1] This is not to say that an edition would not be worthwhile. For notes on the correspondence, see Lackey *1972a*, and my *1974a*, 405, and *1975c*, 92–94. I have not correlated it with the Jourdain correspondence.

book on the history of mathematics. Indeed, the manuscripts in their possession seem now to have completely vanished.[1] In addition, his widow destroyed virtually all her holdings in 1940 and 1946.

Thus it is fortunate that Mittag-Leffler received the notebooks, for while he did not publish the correspondence with Cantor that they contained he preserved them among his papers, and I found them at the Institut during a brief visit in 1968. (My account *1971c* of the Institut's manuscripts is based on a longer visit two summers later.) In addition to the correspondence with Cantor (which is now published as my *1971a*) and the letters with Russell which are our present interest, there is a substantial correspondence with G. H. Hardy[2] and a scattering of letters from many other mathematicians, including G. Frege,[3] J. König, A. Schönflies, J. Venn, G. Peano and E. Zermelo.

Jourdain used these notebooks from 1901 until early in 1911 to draft his letters and paste in the replies he received. Fortunately this period coincides almost exactly with the duration of Russell's involvement in mathematical logic, and only a few letters or drafts appear to be lost. Hence we can form a fairly complete picture of their written discussions, which are presented in sections 5–22 below. The quotations are drawn almost entirely from the notebooks, though the few letters from Jourdain to Russell of the period which are preserved in the Russell Archives have also been used.

The loss of their correspondence of the later years probably does not seriously affect the history of mathematical logic. However, it is a misfortune for the history of Russell's philosophical development during the 1910s and his involvement in the First World War, for their contacts did not cease with Russell's withdrawal from logic. In sections 23 and 24 I have described their later relationship in as much detail as can be drawn from the surviving letters in the Russell Archives and from their published work of the period.

The concluding part of the book contains sections 25–28, of which the first three are written respectively by myself, Russell and Jourdain.

[1] The only other significant items of Jourdainiana of which I am aware are some notes on Boole (now in the possession of J. M. Rollett), and a box of offprints and a manuscript by Millicent Jourdain (partly quoted in Heath *1920a*, 162–171 and related to her Arden *1913a*) in the library of Trinity College Cambridge, Ad.Ms.C86. There are also collections of his letters among the *Nachlässe* of various of his correspondents; some are mentioned in later footnotes.

[2] It deals largely with Cantorian set theory. I have not correlated it with the Russell correspondence. There is also a certain amount of correspondence between Hardy and Russell in the Russell Archives.

[3] These letters are to appear in the forthcoming edition of Frege's correspondence, which will also contain his letters to and from Russell (now kept respectively in the Russell Archives and in the Sammlung Darmstaedter, Stiftung Preussischer Kulturbesitz, West Berlin; the latter source also has a draft letter of January 1914 from Frege to Jourdain).

My contribution draws some conclusions about the correspondence and its significance. Russell's section contains my translation from French of his paper ' Sur les axiomes de l'infini et du transfini ' (*1911a*), which was published very obscurely by the Société Mathématique de France in 1911. Jourdain's section is a small selection of his stories in the *The Granta*, where he expressed in witty style some of the problems that he was discussing with Russell. In the final section, I describe and quote from Jourdain's annotations in his copy of *Principia Mathematica*.

3. *Style, scope and limitations of the commentary*

Although we are primarily indebted to Jourdain for the existence of this correspondence, the text below contains comparatively few quotations from his contributions to it. One reason is that the points he made often correspond closely to parts of his published papers ; but the main reason is that the creeping paralysis from which he suffered severely hampered his handwriting. Members of his family often copied into his notebooks the drafts of his letters, leaving spaces in which Jourdain could insert the mathematical symbols himself. Sometimes he did insert the symbols and sometimes he simply placed in the notebooks the loose sheet containing his draft ; but quite often he did neither, so that several of his copied drafts lack all mathematical expressions and are difficult to understand, let alone publish.

In addition, I have chosen to describe, or quote in footnotes, a few letters or parts of letters from both correspondents which are only of incidental quality. Readers who wish to consult the original documents will find their precise location indicated in the index of the surviving correspondence on pages 214–217.

I have given the date of each letter in context, but have usually indicated the place of writing of only the first of a series of letters from that location. Thus a location is not normally indicated again until the next change of address. Jourdain wrote most of his letters from only four places, but before 1905 and after 1911 Russell moved around fairly often.

The correspondence often took the form of a string of questions from Jourdain followed by a string of answers from Russell. I have usually rearranged the order of quotation or description, and grouped answer with question, in order to unite exchanges on a given topic and so lend clarity to those letters which are particularly intricate and many-sided. But in contrast to this local rearrangement of material, *I have preserved as far as possible the overall chronological order* of the correspondence, rather than regroup it under the different topics discussed. There is

in any rendition of correspondence a need to preserve the sense of chronological advance through the letters; but in this case the topics interrelate so closely anyway that such a dissection would be perilous. In fact, the changes of view on a topic were often caused by changes in more general strategy, and they could only be conveyed through a chronological treatment of the material.

However, a consequence of this method of presentation is that *there is a frequent change of topic*, or return to topics already discussed. Hence readers, or busy reviewers, who wish to locate the discussions of some particular topic *must consult the index of subjects* on pages 223–234 rather than the table of contents, where only principal topics are named in the titles of the sections.

The technical nature of the correspondence inevitably involved an extensive amount of notation, for which a glossary of principal symbols will be found on pages 210–213. They are not always the final notations used in *Principia Mathematica*; in fact, the correspondence sometimes indicates a recent change in notational practice. The index does not exhaust the full range of notations, but Russell usually explained special notations himself in his letters to Jourdain. I have used the same notations in the commentaries.

In quoting from either Russell or Jourdain I have followed certain editorial principles. Without special indication I have completed abbreviations of words, and displayed equations or mathematical expressions which had been written consecutively with prose when clarity of reading was thereby increased. I have also not indicated the foliations or the line-lengths of the letters. But I have represented the omission of passages within quoted extracts by ellipsis dots '. . .', and enclosed any kind of editorial interference not mentioned above within square brackets. Occasionally Russell and Jourdain used square brackets, but (apart from complex symbolic expressions, where I use square brackets conventionally) I have converted those into round brackets.

I have consigned to the footnotes many of the cross-references, citations of relevant published literature and manuscript, letters or points of minor though perceptible significance, and the occasional footnote by Russell or Jourdain themselves. The bibliography became sufficiently large to warrant a section of its own on pages 185–209, with individual items located by the name(s) of the author(s) followed by a dating code or catchword. The references made by Russell and Jourdain themselves have also been converted into this form. Readers who intend to make substantial use of the references should read the detailed description at the beginning of the bibliography of the method of citation of the literature. The bibliography also includes where known the full names and dates of the authors.

An important point of terminology must be mentioned. Some of the methods now used to solve the paradoxes incorporate distinctions between a class and a set. Although theories of this kind do arise in their correspondence, neither Russell nor Jourdain urged this distinction of terminology, and in my commentaries I have followed their preference for 'class' (though they also used 'aggregate' sometimes) with a non-discriminatory connotation. (But I have used 'set theory' to refer in general terms to Cantor's *Mengenlehre*.) In fact, I have followed Russell's basic terminology throughout—but his use of the word 'exist' caused difficulties.

In addition to the orthodox senses of existence in the physical or abstract worlds, Russell also spoke of the 'existence of an individual' in connection with his theory of definite descriptions, and of 'the existence of a class' to refer to its non-emptiness. These senses of 'exist' not only are distinct from but sometimes clash with the orthodox senses, and so in my commentaries I have placed them within *double* quotation marks. However, it seemed too great an editorial interference so to annotate Russell's (and Jourdain's) non-orthodox uses of the word, so I have left them with or without quotes as appropriate. But a conflict with house style arose; for throughout this book single quotes are employed for the usual quotational purposes (including the mention rather than use of words and symbols), whereas Russell and Jourdain normally used double quotes. I decided to convert their double quotes to single quotes, thereby achieving the double aim of indicating that a quotation took place and also of reserving double quotes for the special purpose outlined above. This solution was not perfect, because certain quotations contain sub-quotations, which require the use of double quotes; but I hope that it contains the minimum of ambiguity within its optimum of clarity.

Having decided to use double quotes in commentaries, I have also employed them when speaking of a "type" theory developed by Russell in 1906 which is rather different from that in *Principia Mathematica*. Similarly I refer to "consistent" and "inconsistent" classes whenever I discuss Jourdain's own method of solving the paradoxes.

My interpolated commentaries have been restricted to the contents of the correspondence, as I abhor as impracticable the simultaneous presentation of extensive amounts of documentary material and of detailed analyses of other matters. My remarks presuppose some familiarity with mathematical logic, the foundations of mathematics, and Cantorian set theory, and are confined chiefly to relevant published literature by Russell, Jourdain and others, and to the most obvious points of criticism. Thus *more conclusions can be drawn from these texts than I have explicitly discussed.*

Two particular limitations deserve special mention. Firstly, I have usually restricted to mere mention the discussion of letters and logical manuscripts in the Russell Archives which are relevant to this correspondence, since they deserve a substantial amount of analysis for which this is not the appropriate place. Secondly, I have avoided discussing topics from the viewpoint of some modern or recent treatment of them. For general historiographical considerations seem to me to argue in favour of such an approach. I believe that it is not only dangerous but also *basically undesirable* to discuss a period historically in terms of a modern or at least later standpoint. The use of a later technique automatically excludes the writing of the history of its discovery, and prevents the analysis of the period in question in terms of the *lack* of such a technique. Thus it becomes impossible to construct the ' ignorance situation ' of the historical figures ; their ignorance of the advances made by those who came after them *but before us*. Again, the structure of priorities in the historical period is very likely to be distorted into one identical with or close to our own, so that anything important *now* had to be important *then*, while the problems of no significance today do not receive the degree of historical interest appropriate to their (quite possibly) higher status during the period in question.

Of course, there is no objection to the use of an historical period in this way for other purposes ; indeed, it is quite often the way in which scientific progress is made, and it must be the way in which knowledge is treated *as* knowledge without reference to its historical genesis. It may also provoke questions for the past from its own special features. But by definition it is not the route to historical evaluations of the subject matter. The history of a period is its pre-history—its own history—not its post-history.

4. *Acknowledgements*

The copyright on Russell's unpublished letters and writings rests with Res.-Lib. Ltd., McMaster University, Hamilton, Ontario, Canada, and on Jourdain's letters and manuscripts with his widow (now Mrs. L. Breglia). I acknowledge with gratitude their permission to publish documents under their jurisdiction.

Russell's side of the correspondence is in the Bertrand Russell Archives at McMaster University, and I acknowledge the great assistance afforded to me by K. Blackwell and his staff. Jourdain's notebooks are kept in the Institut Mittag-Leffler, Djursholm, Auravägen 17, Sweden, and similarly I am grateful for the help provided by Professor L. Carleson and his assistants.

For permission to publish my translation of Russell's paper ' Sur les axiomes de l'infini et du transfini ' I am grateful to the Société Mathématique de France. I am also indebted to *The Granta* for allowing me to republish some of the contributions they received from Jourdain, and to H. F. Norman for allowing me to see the annotated pages of Jourdain's copy of *Principia Mathematica*.

For information of a documentary or factual character I am grateful to all the institutions and persons named above, and also to R. W. Clark, the late Sir Edward Collingwood, J. W. Dauben, J. M. Rollett, and the College office and library of Trinity College, Cambridge. My wife helped considerably in the preparation of the manuscript, and R. Bunn (University of British Columbia), J. Corcoran (State University of New York, Buffalo) and G. H. Moore (University of Toronto) provided substantial criticisms of the technical and historical content of draft material. Further acknowledgements in connection with the photographs are given on the captions.

January, 1975.

The Correspondence

5. *March 1902–March 1903 : irrational numbers and the real line*

The earliest surviving letter is by Russell, and was written from his wife's family home at Friday's Hill, Haslemere, Surrey. It is dated 16 March 1902, when he was nearing the completion of *The Principles of Mathematics*, and appears to be a reply to a letter from Jourdain of which there is now no trace. One passage reads :

> With regard to irrationals, the view I adopt is that real numbers (both rational, so called, and irrational) are segments of rational numbers. This view is set forth, but not adopted, by Peano.[1] My ground for adopting it is the failure of the existence-theorem in all other theories of irrationals. There is not any rational number corresponding to an irrational Dedekind *Schnitt*, and it is a gratuitous assumption that there is an irrational number having this property. Cantor professes to prove the existence-theorem, but his proof is fallacious. It is easy to prove that the series of segments is continuous, and thus the continuum is generated in a purely arithmetical manner.

In *My Philosophical Development* Russell claimed that his definition ' puts an end to a puzzle which had perplexed mathematicians ever since the time of Pythagoras ' (*1959a*, 72). But his own account of the problem in *The Principles* calls the claim somewhat into question. For there he asserted that Dedekind's (and presumably anyone else's) theory ' is designed to prove the arithmetical existence of irrationals ' (art. 267). But, as Dedekind had made clear in the preface to his pamphlet *1872a* on continuity and irrational numbers, his purpose had been to construct a theory of continuity by means of the cut axiom for the purpose of proving the existence of *limits*, limits whose values (rational or irrational) were already defined. The association of this continuity axiom with a definition of the irrationals is certainly an assumption, but to describe it as ' gratuitous ' seems rather to miss the

[1] Russell now cited Peano *1896a* ; see especially art. 8. He mentioned this paper also in *1903a*, art. 258 and note to art. 261.

point. However, in *The Principles*, art. 266 Russell did improve the formulation of the axiom.

Russell was also not quite accurate in his assessment of Cantor's theory, in which the irrational was associated with a fundamental sequence of rationals. More precisely, Cantor expressed the definition of a fundamental sequence $\{a_n\}$ of rationals as follows (*1883b*, art. 9):

$$\text{'}\lim_{\nu=\infty}(a_{\nu+\mu}-a_\nu)=0 \quad \text{(for arbitrarily composed } \mu\text{)'}. \tag{5.1}$$

Then he associated with it a value b so that

$$\text{'}\lim_{\nu=\infty} a_\nu = b\text{'}. \tag{5.2}$$

In *The Principles*, arts. 269–270, Russell seems to have read into this procedure the *assumption* of the defined b by Cantor and thus the introduction of a vicious circle. But Cantor was well aware of this danger, for he prefaced his definition above with an explicit discussion of this point and used the verb 'associate with' (*zuordnen*) to describe the relationship between $\{a_n\}$ and b in a manner to avoid vicious circularity. The difficulty of this theory concerns the interpretation of two fundamental sequences which define the same real number. Cantor defined this possibility as follows. Let b and b' be associated with fundamental sequences $\{a_n\}$ and $\{a_n'\}$; then $b=b'$ if

$$\text{'}\lim_{\nu=\infty}(a_\nu-a_\nu')=0\text{'}. \tag{5.3}$$

But then either we have to prove rather than assume that this equality is an equivalence relation for it to qualify as an equality relation for arithmetic,[1] or else we must define the associated real number as the class of sequences satisfying (5.3).

Russell might have questioned in *The Principles* Cantor's construction of higher orders of number (as Dedekind did in the preface to his *1872a*). He did criticise Cantor's theory for the idealist cast of the process of association (art. 270), on the same ground as his criticism of Peano's use of a 'principle of abstraction' to abstract a property from a collection of objects satisfying an equivalence relation (arts. 108–111); that there is no guarantee of the uniqueness of the property so associated

[1] In *1883b*, art. 9, Cantor asserted that the problem of different sequences being associated with the same real number could be solved by the 'specialisation' of some base sequence for the number; but this kind of solution would appeal only to those who shared his idealist cast of mind in contexts such as this. Cantor's account of his theory of irrationals in *1872a*, art. 1 (which Russell did not cite) is better than its successor; for commentary on it, see Frege *1903a*, arts. 68–85, and Dauben *1971a*, 202–208. Compare also Heine's theory in *1872a*, 175 and 177.

or abstracted.[1] He had as a theorem his own principle of abstraction, which stated that a transitive symmetric relation can be expressed as the product of a many-one relation with its converse, and he showed that this theorem reduced definition by abstraction to definition in terms of equivalence classes.[2] So he adopted the view which he reminded Jourdain that Peano eschewed; that real numbers be defined as segments of rationals (see arts. 258–259), where 'segment' refers in Cantor's way to the class of predecessors of a non-initial member of a well-ordered class (see Cantor *1897a*, art. 13, and Russell *1901c*, art. 3).

This theory obviates the need for an equivalence relation to be imposed on the definition of the real number; but then so does Dedekind's theory, thus lending some merit to Dedekind's assumption which Russell had described to Jourdain as 'gratuitous'. Russell's role in the development of the irrationals seems enigmatic, for the element of his misrepresentation of the problem and of his predecessors' work on it has to be set against his own theory. Not everyone has followed him in his claims; indeed, one of his earliest critics was Jourdain.[3]

One factor which may have influenced Russell was his various uses of the word 'existence' for classes, referring it both to their ontological (or abstract) existence and also (if appropriate) to their non-emptiness. This latter sense was introduced by Peano (*1897a*, def. 15) and was adopted by Russell in *The Principles*, art. 25. As we warned in our discussion of Russell's terminology, a distinction will be made in these commentaries; existence in the ontological sense will be taken as the orthodox sense for the word, while the non-emptiness of a class will be described as "existence", with double quotation marks. Thus we shall say that Russell may have associated the existence of a class with its "existence", and called both properties its 'existence'.

In *The Principles* Russell tended to assume the existence of classes, although the presence of his paradox and the possibility of subsuming classes under propositional functions kept the assumption from naivety. Thus he was clear about the distinction between nothing and the empty class (arts. 69, 73 and 79). However, he seems to have been uncertain about the existence but non-"existence" of the empty class (arts. 25,

[1] On Peano's use of this principle see, for example, *1894a*, art. 38. He defined integers by the well-known 'Peano axioms' (first in *1889a*). For commentary, see Cassina *1954a*, Vuillemin *1968a*, ch. 5, and *1972b*, and Kennedy *1974a*.

[2] First proved in Russell's *1901c*, art. 1, ✳6.1 and ✳6.2. See also *1903a*, arts. 157 and 210, and *PM*, vol. 1, 442. For a discussion of definition by abstraction, see Scholz and Schweitzer *1935a*.

[3] See especially Jourdain's *1906a*, *1908a*, 203–205, *1913b*, 494–496, and *1916b*, 419. Compare Wang *1966a*, 340–346, esp. p. 341 (also in *1974a*, 237): 'The foundation of arithmetic is more trustworthy than that of set theory'. Darbon accepts Russell's views on irrationals in *1949a*, 80–82.

36 and 106). Potential difficulties arise also in *Principia Mathematica*, where a class does not exist because of its subsumption under a propositional function but will " exist " if non-empty. We note that he stated there both that ' the following theory of classes . . . avoids the assumption that there are such things as classes ' (vol. 1, 187) and also that ' when a class α is non-null, so that it has one or more members, it is said to *exist* ' (p. 216).

A similar situation applies in mathematics to existence-theorems, which may affirm the existence of an object (for example, of a point on a curve at which some situation applies) or of a process (such as a means of reducing a mathematical form to a special kind of that form), but which might instead assert the " existence " of some class. Russell seemed to regard existence-theorems in his predecessors' theories of irrationals as being only of this latter kind. Thus, while his criticisms can be justified, they do seem to imply that no other view of existence-theorems, and thus of definitions of irrationals, is possible.[1]

In his letter of 16 March Russell also reported on the progress of his work :

> I intend publishing my lectures together with other material in a joint book with Whitehead, but probably not for two years or so. I also intend publishing a book (for which I have done the bulk of the work) on the philosophical aspects of the subject, and on controversial points. With regard to method of discovery, there is little to be said ; I know of no one method, but my usual plan is to take established pieces of reasoning and analyse them with all possible care and minuteness.
>
> I dare say Mr. Whitehead or some one else would lend you his notes of my lectures, if you are anxious not to wait for their publication.[2]

The forthcoming book was of course *The Principles*, which was completed in the following May (according to Russell *1967a*, 151). But the ' joint book with Whitehead ' was to take more than seven years,

[1] Note especially Russell *1903a*, art. 474. Jourdain contrasted Russell's use of the word ' existence ' with the mathematicians' use in various places, especially *1906c*, 132–136, *1910a*, 113–114, *1912a*, 225–231, *1913b*, 496, and *1917b*, 142–149.

[2] No manuscripts of Russell's lectures of 1901–1902 seem to survive in the Russell Archives, except perhaps a page or two entitled ' Lecture II. Logic of propositions '. The undated manuscript ' Proof that no function takes all values ' appears to refer to the lectures in two places. Whitehead's papers were burnt on his instructions after his death (Schilpp *1951a*, 749), so that no information is available from that source.

In this letter Russell also referred Jourdain to ' the best books ' on the foundations of geometry : Pieri *1898a* (actually a paper), Pasch *1882b*, Peano *1889b* ' and various articles in Rivista di matematica '. But he did not mention Hilbert *1899a*.

rather than two ' or so ', before *Principia Mathematica* began to be delivered to the Cambridge University Press.

On page 3 above we quoted from an account by Russell of a visit to Jourdain in November 1902. Very likely it was the visit to which Jourdain referred in his first surviving letter, dated 17 November from Chesterton Road, Cambridge, enclosing ' some papers you had written on the paradox ' and had left behind. Jourdain also asked if a particular passage in Cantor's *Zur Lehre vom Transfiniten* of 1887 anticipated Russell's definition of the cardinal of a class as the class of classes in one–one correspondence with it. In fact the passage is an anticipation of Cantor's own 1895 definition of cardinals in terms of the double abstraction from a class of the nature of its members and then their ordering,[1] and in his reply of 22 November from Cheyne Walk, Chelsea, Russell rejected Jourdain's suggestion :

> I have read Cantor's Zur Lehre vom Transfiniten ; but I do not think the passage you allude to can be taken as a definite statement of the logical definition of numbers. This definition however (as I have learnt since giving my lectures) is quite explicitly set forth by Frege, Grundlagen der Arithmetik, Breslau, 1884 ; see also his Grundzüge[2] der Arithmetik, Jena, 1893. He is a very able man : I have been corresponding with him for some time about my paradox.

Russell's confession of ignorance of Frege's work seems to contradict the opening of his first letter to Frege, dated 16 June 1902. There he claimed to have been acquainted with the *Grundgesetze* for eighteen months, well before the presentation of his lectures (van Heijenoort *1967a*, 124). Presumably his initial understanding of the book had been slight ; he hinted as much to Jourdain later (see page 133 below).

Jourdain's main research interest at this time was in the theory of functions. He seems to have enclosed with his letter of 17 November a manuscript on this subject, for in the reply Russell disclaimed competence in the subject but encouraged Jourdain to consult Whitehead :

> I do not know enough of the Theory of Real Functions to offer any suggestions or critical remarks, but it certainly seems as though

[1] See Cantor *1887–88a*, art. 1. The passage is apparently the substance of a lecture of September 1883, where he outlined his theory of order-types and cardinals as single and double abstractions from classes. His theory became better known in its more detailed appearance in *1895a* and *1897a*. In this letter Jourdain mentioned that Whitehead had borrowed his copy of Cantor's *1887–88a*.

[2] Sic : read ' Grundgesetze '. Russell cited only the first volume (*1893a*), for the second did not appear until 1903. For Frege's definition, see art. 40. Russell first published his definition in *1901c*, art. 2 ; see also *1903a*, art. 111.

the line of thought upon which you have started would prove well worth pursuing. Do ask Whitehead about his way [in *1901a*] of generating functions of a real variable out of the Logical Calculus, as I do not know whether his existence-theorems come within the scope of your argument or not.

It is not known whether Jourdain took Russell's advice to consult Whitehead on this subject, but contact with Whitehead on another matter is recorded in Jourdain's next surviving letter, dated 14 March 1903. He had prepared a manuscript in French for the *Bulletin des Sciences Mathématiques* and sent it both to Russell and Whitehead for comment. It does not seem to have been published or to have survived, but apparently it dealt with the theory of functions and involved a problem which he discussed in his letter : the ' intercalation ', or interpolation of real numbers into a sequence of such numbers already specified. He wrote to Russell :

I have found a precedent for the intercalation of real numbers in the ordinal numbers (types of well-ordered $\aleph_0$'s)

$$1, 2, \ldots, \nu, \ldots, \omega, \omega+1, \ldots, \omega \cdot 2, \ldots, \alpha, \ldots$$

by which I tried to construct (in the French paper) an argument- and function-domain . . .[1] I intercalate like this :

(1) Consider the gap $\nu \ldots \nu+1$. Put in an aggregate of type 3, viz. ν, $\nu+\frac{1}{2}$, $\nu+1$;[2] treat sub-gaps in the same way.

(2) Put in $\nu \ldots \nu+1$ a type 4, and so on.

(3) Order elements, by determining that

$$\nu+\tfrac{1}{2} = \nu+\tfrac{1}{4}, \ldots$$

We get ' rational numbers '.

(4) Get irrational numbers by, say, the segment-method. I suppose Cantor must no longer be used (shall I find anything on Cantor's and Dedekind's faults in your new book that I hear is just going to appear ?)

(5) Prove uniqueness of addition, subtraction, . . . (it is easy to see how these are to be defined, by a conglomerate of definitions like Cantor's for types [in his *1883b*, art. 11]).

Now (4) troubles me especially. Perhaps you would be kind enough to suggest how I can shortly say what I want without making any very bad blunders about irrational numbers. I

[1] Jourdain now cited and quoted from Cantor *1883b*, 8 (*Papers*, 161). All subsequent ellipsis dots in this extract are Jourdain's own.

[2] ' Type 3 ' refers to an ordered triple ; compare Cantor *1895a*, art. 5.

should very much like to have your opinion as to the possibility of constructing a useful real number-system in this way.

In reply on 17 March, Russell wrote :

> Concerning intercalation, some general remarks may be made. (1). Cantor, as you know, errs in the difficult matter of the existence of limits. You will find a discussion of his errors about irrationals in my book, which I shall allow myself the pleasure of sending to you. (2). You cannot, as Cantor thinks, simply add rational or real proper fractions to true integers, whether ordinal or cardinal, finite or transfinite.[1] If you endeavour to construct a logical definition of such addition, you will see at once that it is impossible. What we call $1+\frac{1}{2}$ is really $1 : 1+1 : 2$, and the $+$ is not the same operation as in addition of integers. Or, if the 1 and the $\frac{1}{2}$ are real numbers, and therefore *not* rational numbers, the $+$ has again a different meaning. The rational numbers cannot be extended beyond the finite series, but the real numbers, or at least a serviceable analogue, can be. (3). There is, of course, no theoretical difficulty in constructing a well-ordered series of continuous series. Take, e.g., the classes of rationals which are the real numbers, and add to every such class the class $\iota\alpha$, where α is a certain finite or transfinite ordinal. By varying α, we get a well-ordered series of continuous series ; and this gives us what you want, the whole series of real numbers (or of such only as [sic] are proper fractions if we prefer this) intercalated between any two integers. (4). But such a method, though valid, is not at all elegant, being too artificial and external.

The approach described by Russell under (3) seems to proceed by intercalating the unit class of the ordinal α into each class of rationals which defines a real number. Since the reals form a continuous series of classes of rationals, the series of extended classes is also continuous. The variation of α generates a well-ordered series of such extended series. He expressed his reservations of the method at the end of the above extract, and continued his letter with an alternative approach to intercalation :

> The following seems preferable. Let α be any transfinite ordinal, and consider all classes of ordinals none of which contain α or any greater ordinal (i.e., if M = less than, consider Cls'$M\alpha$). We

[1] Cantor mentioned avoiding such procedures in *1889a*. For criticisms of this paper, see Frege *1903a*, art. 74.

may arrange these on the following plan : of two classes whose β smallest terms coincide, while their $(\beta+1)^{\text{th}}$ terms differ, that one is to come first in which the $(\beta+1)^{\text{th}}$ term is the smaller. If α be ω_0 and if we omit all finite classes of integers, this plan gives a continuous series ; the η contained in it is composed of such classes as contain all except a finite number of the finite numbers.[1] If α be any other ordinal, we shall still get a series ; and by omitting some of its terms, the series can be made compact, and can acquire many of the properties of continuous series. The method, I believe, is a powerful one for generating new types ; but I have not yet carried the investigation of it very far. For what you want, it will be best to take your θ from classes of finite integers, and get a well-ordered series of θ's by adding $\iota\alpha$, where α is any transfinite ordinal, to every such class, to get all the numbers between α and $\alpha+1$. The integer α is replaced by $\iota\alpha \cup$ No fin.

Whether such a method can ever have any utility, I do not know. I have not found any way of judging beforehand what will be fruitful, and what not.

The method was indeed fruitful, for it was generalised in *Principia Mathematica* into ' the principle of first differences ', which was used in procedures such as ordering the sub-classes of a class (✳170–✳171 ; see also ✳276). Its significance here is greater than may be immediately apparent, for it exemplifies Russell's formulation of set-theoretic techniques in terms of *ordered* classes (or ' series ').

Russell was fascinated by relations from an early stage of his researches, possibly through his study of geometry[2] and also of Cantor's set theory. In 1899 he wrote a manuscript entitled ' The classification of relations ' (preserved in the Russell Archives) which discusses different kinds of relation. At the Paris Congress of 1900 he read a paper *1901b* on order and position in space and time. After his conversion to Peanoism there he must have noticed the limited extent to which Peano had developed the logic of relations,[3] for immediately

[1] ' η ' is Cantor's symbol for the rational order-type. Later in this letter Russell used ' θ ' for the order-type of the continuum. Attention is recalled to the glossary of principal notations on pp. 210–213 below.

[2] The theme of Russell's dissertation *1897b* on the foundations of geometry is that geometry deals with space and the relationships between objects in space. The influence of geometry (especially projective geometry) on Russell's interest in relations is discussed a little in Bonfantini *1970a*. On the controversy over Russell's *1897b* see Dambska *1974a*. Another source of Russell's interest in relations may have been his desire to refute Bradley's internalist conception of relations (as is argued in Jager *1972a*, 59–76 *passim*).

[3] See, for example, Peano *1894a*, arts. 23–30, esp. art. 30, where he defines only the class of referents of y under a relation R (Russell's $\dot{R}'y$ in *PM*, ✳32.13).

afterwards he wrote a paper treating relations in detail according to Peanesque principles.[1] In 1902 he published a succeeding paper on well-ordered series, of which a prominent feature was the analysis of ' ordinally similar ' relations, that is, relations R_1 and R_2 for which there exists a one–one relation Q such that

$$R_1 = \breve{Q} R_2 Q.^2 \tag{5.4}$$

Russell discussed ordinal similarity also in *The Principles*, arts. 231 and 299, and by the time of *Principia Mathematica* he had greatly extended the idea into his most substantial mathematical contribution to the project; Part IV, on the theory of ' relation arithmetic ', a generalised theory of ordinals defined as classes of ordinally similar relations, of which the principle of first differences was a part. His treatment of Cantorian set theory and the foundations of mathematical analysis was dominated by this style of working, and was probably inspired by Cantor's stress on the different possible ways of ordering infinite classes.[3] In fact, it was a translation of Cantor's work into a theory of ordered classes as fields of their generating relations. It was a major influence on Jourdain,[4] and we shall see it recur in their correspondence.

6. *May 1903 : existence-theorems for cardinals and ordinals*

On 10 May Jourdain acknowledged the receipt of the copy of *The Principles* which Russell had promised him in March. After suggesting some additional bibliography for the book, including H. Méray's anticipation of Cantor's theory of irrationals,[5] he asked a question about the so-called ' arithmeticisation ' of analysis :

[1] Russell *1901c*. The manuscript survives in the Russell Archives in both English and French versions ; the former is dated October 1900.

[2] Russell *1902a* ; see esp. pp. 16–18, and compare *1903a*, arts. 251–253. The manuscript survives in the Russell Archives in both English and French versions ; the latter is dated July–August 1901. Note in *1902a*, 15, ✳1.35 a correction to the definition of ω given in *1901c*, art. 3, ✳1.1, which was not incorporated in the translation of *1901c* in *1956a*, 15.

[3] See especially Cantor's *1883b*, and also his manuscript of 1884–1885 which was first published in my *1970a*, 82–101. For an extensive study of Cantor's and others' work on order, see Huntington *1917a*.

[4] See especially Jourdain's *1905e* on the theory of functions ; on p. 184 he acknowledged Russell's influence. Jourdain discussed the ideas presented in this paper in several letters to Russell of this period, and stated in *1919d*, 167 that it was written in 1902 and 1903. His *1903a* and *1904a* on the cardinality of the class of integrable functions relate to it.

[5] Presumably an allusion to Méray *1869a*. Jourdain also mentioned Biermann *1887a* and Harnack *1884a* to Russell, in connection with *The Principles*, arts. 268 and 277 respectively.

Is it correct to say that the great result of Cantor's *later* works and your has been to shew that the usual 'arithmetical' way of developing analysis (Weierstrass, Cantor (up to 1882), and nowadays Pringsheim, Stolz, Schönflies, Jordan, . . .)[1] (in which, starting from the idea of integer, the generalisations of number are defined, and then aggregates treated as selections from the real number-'continuum') contains a ὕστερον πρότερον [begs the question], inasmuch as the notions of limit-point, . . ., which are introduced at the *end* of the above, are really fundamental in the theory of (ordinal) integers? If this (or something like it) is so, I wish that it could be written large in (say) vol. ii, because failure to grasp this point was, I think, the chief cause of my not understanding parts of your lectures; and there may be other people as ignorant as I was. (I mean people who have swallowed 'arithmetical' criticism uncritically, and whose knowledge has the upper limit of 1882.)

Russell's reply of 14 May from Churt, Farnham, is among the more cryptic of his responses, perhaps because it dealt with questions on which his views at this time were particularly uncertain :

Many thanks for your letter and for the information you give me. I have noted in the margin all the points you mention. I knew nothing of Méray, whose name even is unfamiliar to me.

As regards your question concerning the 'arithmetical' development of analysis, I do not know how to answer simply. The points involved are these : All the properties of real numbers which occur in analysis depend upon (1) the derivation from an ω (the integers) through an η (the rationals, which are 1–1 relations of integers); (2) the quite special fact, upon which, in this region, the peculiarities of numbers depend, that the finite integers themselves (ordinal or cardinal as we choose) are the indices of powers of relations, and in particular of powers of the relation of a number to the next after it. It is this that is expressed or involved in such propositions as

$$aN^b c \;.\; = \;.\; a+b=c$$

where

$$xNy \;.\; = \;.\; x, y \in \mathrm{No}.\; x+1=y \quad \mathrm{Df.}^2$$

The above two facts are those upon which *ordinal* Arithmetic depends; it is those [properties] dependent only upon (1) that can be proved without the use of numbers of any kind.

[1] The ellipsis dots in this extract are Jourdain's own. For a survey of the literature to which he was referring here, see Pringsheim *1899a*.

[2] N is the successor function, and N^b its b-fold iteration. For the first '=' in the previous line, we should presumably read '≡'.

Russell seems to be saying that the real numbers rest on the construction of a rational order-type η (that is, one which is denumerable, dense, and lacking first and last members) from a progression ω without specification of the progression *as* the class of finite ordinals, but that the finite integers may be distinguished from other progressions by their role as powers of the successor relation N. He also took cardinals and ordinals as indistinguishable in the finite range, but in continuation he suggested grounds for taking cardinals as prior :

> But the following points appeared to me to necessitate beginning with the *cardinal* theory. (a) The existence-theorem for finite integers can only be so proved. This is derived thus :
>
> $\cdot 1 \quad 0 = \iota\Lambda \quad \text{Df} \qquad \cdot 2 \quad x \ni (x0'x) = \Lambda$
>
> $\cdot 3 \quad \cdot 1 \cdot 2 . \supset . \exists 0 \qquad \cdot 4 \quad u \in \text{Cls fin} . \supset . \exists - u$
>
> $\cdot 5 \quad \cdot 4 . \supset : n \in \text{Nc fin} . \supset . \exists (n+1).$
>
> Thence
>
> $$\alpha_0 = \text{Nc}\,(\text{Nc fin}) . \supset . \exists \alpha_0.$$
>
> Also, from the relation of greater and less among cardinals, we get $\exists \omega$, which, so far as I know, is not otherwise demonstrable ; and $\exists \eta$ is obtained also from the cardinals. But apart from these existence-theorems, the ordinal theory seems independent ; and I do not think it involves a *ὕστερον πρότερον*, unless the usual mistake is made of assuming the existence of irrationals.

The relationship between cardinals and ordinals was a controversial matter among foundationalist mathematicians. Some of them, including Dedekind in his *1888a*, art. 161, took the ordinals to be prior ; but others, especially Cantor in *1883b*, art. 2 after his work on the different kinds of ordering of infinite classes, stressed the primacy of cardinals. In *The Principles*, as in the passage above, Russell asserted that ' there seems, in fact, to be nothing to choose, as regards logical priority, between ordinals and cardinals, except that the existence of the ordinals is inferred from the series of cardinals ' (art. 230 ; see also arts. 122–123, 232 and 243). As with irrational numbers, he saw the existence of cardinals or ordinals in terms of the non-emptiness of appropriate constructed classes. Thus he had to proceed in quite a different style from Cantor, who claimed the existence of both his irrationals and his transfinite numbers on the formalist ground of the (alleged) consistency of their construction. Indeed, Cantor even called his transfinite numbers ' new irrationalities ' to emphasise the connection :

> In a certain sense the transfinite numbers are themselves *new irrationalities*, and in fact in my opinion [this] is the best method of defining the *finite* irrational numbers, quite similar to, I might even say in principle the same as, my method described above for the introduction of the transfinite numbers [by defining ω as the limit ordinal of the series of finite ordinals]. One can say unconditionally: the transfinite numbers *stand or fall* with the finite irrational numbers . . .[1]

By contrast, for Russell 'freedom from contradiction can never be proved except by first proving existence' (*1910d*). But he shared with Cantor the belief in the existence of an infinite class. Indeed, when C. J. Keyser proposed in 1904 that the axiom of infinity was required *as* an axiom,[2] Russell quickly replied with the sketch of an argument similar to the one above, which allegedly proved the principle of mathematical induction and thence (for him) the "existence" of the series of finite integers and of $\aleph_0$ (with 'infinite' understood in its non-inductive sense).[3] He went further with Jourdain, to assert the "existence" also of the class η of rationals. As we shall see, he was to claim the provability of the axiom of infinity for some time to come.

7. *March–April 1904: well-ordering and "inconsistent" classes*

Over the winter of 1903–1904 Russell was heavily occupied with activities outside mathematics and logic, especially the campaign for Free Trade. His correspondence with Jourdain for this period appears to have been confined chiefly to three letters of acknowledgement of offprints at Cheyne Walk, Chelsea on 8 October 1903, and 18 January and 15 March 1904. The second of these letters contains the following passage on both of his current interests:

> I quite agree that certain aggregates are odd, like divergent series, and have to be treated specially. The Contradiction results from

[1] Cantor *1887–88a*, art. 1 (*Papers*, 395–396). For commentary, see Dauben *1974a*, and compare Russell *1903a*, art. 300.

[2] Keyser *1904a*, 539; compare *1903a*. Some measure of priority may be due to Burali-Forti (*1897a*, prop. 29; compare *1896a*), and perhaps also to Cantor's letter of 28 August 1899 to Dedekind (Cantor *Papers*, 447–448). Van Heijenoort is in error in *1967a*, 199 in giving priority for the axiom to Zermelo *1908b*, axiom VII.

[3] Russell *1904a*. Compare *1903a*, art. 339, where he accepted the idealist arguments of Bolzano (*1851a*, art. 13) and Dedekind (*1888a*, art. 66); Frege's treatment of mathematical induction (first in his *1884a*, arts. 79–84) might have influenced him. Intuitionists would reject Russell's proof even while accepting mathematical induction. Keyser rejected it in *1905a*, and Russell rejected it himself in *1919a*, 137–141.

a case of an odd aggregate. As to the finding of criteria for inconsistent aggregates, and as to how to treat such aggregates, probably generations of labour will be necessary.

Just now I am wholly occupied with propaganda in favour of free trade: I fear that protection would be the knell of England's greatness, both material and moral. Consequently I am off philosophy and mathematics for the moment, but I shall get back to work in the spring.

Spring saw a resumption of their intensive discussions. Jourdain seems to have sent Russell some of his work in January, for Russell replied in a postcard of 21 January that he was still too busy with other matters to look at it. But in the letter of 15 March he announced himself free for mathematics again (and also commiserated on Jourdain's failure to win the Smith's prize at Cambridge). Two days later Jourdain sent him a long letter from his home at Yateley, Hampshire, which began with a repeat of his January result: an argument to show that only denumerable classes of numbers could be constructed by arithmetic operations, and thus that it was impossible to prove Cantor's Continuum Hypothesis (that the non-infinitesimal continuum contained $\aleph_1$ members) by directly constructing an ω_1 sequence of real numbers.

Jourdain published his argument soon afterwards in his *1905a*, 43–44, but the version that he sent to Russell reads as follows:

Every real number x that we can determine actually is determined by a sequence of rationals $u_1, u_2 \ldots$ where

$$u_n = f(n, v_1 \ldots v_m) \qquad [(7.1)]$$

where $v_1 \ldots v_n$ are rationals and f denotes a function formed by a finite number of operations on $n, v_1 \ldots v_m$. For every *definite* (constant) real number $\left(\text{e.g., } e, \text{ where } u_n = \sum_{r=1}^{n} \frac{1}{r!}\right)$ can be thus represented, n and m being finite.

Now, the cardinal of all such functions u_n where m is fixed, is evidently $\aleph_0$. We get all such functions by successively making $m = 1, 2, \ldots$ and the cardinal of all these functions is

$$\aleph_0^{\,1} + \aleph_0^{\,2} + \ldots = \aleph_0 \times \aleph_0 = \aleph_0.$$

We can state this result: The cardinal of all real numbers which can be expressed as limits of series (of type ω) of rationals, *the law of the series being known* $\left(u_n = \frac{\alpha_n}{2^n},\ \alpha_n = 0 \text{ or } 1, \text{ is } \textit{not a definite law}\right)$ is $\aleph_0$. Thus by actually constructing each number (of course

this actual construction need only be indicated in a theoretical paper) we can only get $[\aleph_0]$ and can never exhaust a $2^{\aleph_0}$ or fill a $\aleph_1$.

Russell delayed his reply until 12 April, after a consultation with Whitehead at Trinity College, Cambridge :

> I showed your letter of March 17 to Whitehead and we discussed it together. We agreed that it is not obvious (though it may be true) that the cardinal number of all functions u_n of the form
>
> $$u_n = f(n, v_1, v_2, \ldots v_m)$$
>
> is α_0 (I can't make Alephs) and that therefore the proof of your proposition about actually well-ordering a 2^{α_0} was not yet convincing, though of course it may become so.[1] But so many things that seem obvious turn out false, that we have got into the way of being very careful about assuming things.

The reaction of Russell is surprising, since today it is obvious that any process of finite construction will yield only a denumerable class ; compare, for example, the proof that primitive recursive functions form a denumerable class (Kleene *1952a*, 272–273). Here, only the rationals are constructible by Jourdain's method.

The bulk of Jourdain's letter of 17 March was given over to a slight reworking of an argument presented in his *1904b* (of which he had just sent an offprint to Russell), for apparently it had ' not appeared convincing to Mr. Whitehead and Mr. Hardy '. We shall have to dwell a little on it, since it was to provide a lasting point of discussion between Russell and Jourdain.

In his paper Jourdain attempted his first proof of Cantor's well-ordering principle. Cantor defined in *1883b*, art. 2 a class M to be ' well-ordered ' if it possessed a first member with respect to the ordering, and if any sub-class of M which had successors in M had an immediate successor in M. In his *1897a*, art. 12, he repeated the definition and then proved that each non-empty sub-class of M had a first member. Jourdain followed Russell in *The Principles*, art. 296 in taking this theorem as an alternative definition of well-ordering, and then claimed the following theorem : A necessary and sufficient condition for M to be well-ordered is that it contains no sub-class of type $^*\omega$ (the inverse of the order-type ω of the finite ordinals).

[1] Cantor proved in *1895a*, art. 4 that the cardinality of the continuum was $2^{\aleph_0}$ (in Russell's notation). This is why Russell used ' 2^{α_0} ' as a name of the order-type of the continuum.

The proof of the necessary condition follows from the definition. For sufficiency, if M were *not* well-ordered, then at least one sub-class P would not possess a first member. Hence (for Jourdain) P would contain a sub-class Q of order-type $^*\omega$. Let q be the largest member of Q. Then Q would be contained in the sub-series A of M which starts with 1 and ends with q. But A is well-ordered, and so cannot contain Q; hence M is well-ordered (*1904b*, 65–66; notations added).

Jourdain's proof is of the kind which incorporated unintentional uses of an axiom of choice, in this case in the derivation of Q within M.[1] As we shall see, such axioms were still to make their controversial appearance in the world of mathematics. But his belief in his proof led him now to a standpoint concerning the paradoxes which was different from Russell's. For whereas Russell denied that the class W of 'all' ordinals was a class, Jourdain admitted W and even claimed it to be well-ordered. However, in view of Burali-Forti's paradox of the greatest ordinal,[2] he denied it an order-type and thus an ordinal or cardinal. In his letter of 17 March to Russell he described his theory and related it to his published proof of the well-ordering principle as follows:

> I begin with some general preliminaries. The concept of Cls I take from you ($x \varepsilon \phi(x)$). Thus the aggregate W of *all* ordinal numbers is a Cls (although we shall see that Nc'W does not exist).
>
> We next define the relation 'sim' . . ., 'series' and then the 'well-ordered series' $\mathfrak{W}$ which is that simply ordered aggregate (which can afterwards be proved to be well-ordered, compare § 3 of my paper[3]) such that every segment of it is well-ordered, and M is a well-ordered series implies that M is ordinally similar to a segment of $\mathfrak{W}$ or to $\mathfrak{W}$ itself. (We *afterwards* prove that $\mathfrak{W}$ is ordinally similar to W, the series of all ordinals, arranged in order of magnitude.)
>
> All this precedes the definitions of cardinals or ordinals. I now define a *consistent* class u as a class u such that no part of u is

[1] In *1906a*, 35 (*1973a*, 142) Russell rejected Jourdain's proof for its assumption of an axiom of choice. Some theorems on equivalent definitions of a well-ordered class are provable without use of an axiom of choice, but not when they are asserted of *any* class; see Sierpinski *1928a*, 162, or Fraenkel *1953a*, 246–247.

[2] Burali-Forti *1897a*. He defined a property called 'perfectly ordered' instead of using Cantor's well-ordering; but in *1897b* he acknowledged the excessive narrowness of his definition, while stressing that his paradox was not affected by the change.

Cantor has priority for this paradox, and the source usually cited for this information is F. Bernstein *1905a*, 187. But Bernstein in turn cites Jourdain *1904b*, 70, who refers to a letter from Cantor which is now published in my *1971a*, 116–117. Jourdain mentioned this part of the letter also in *1905g*, 466.

[3] Jourdain *1904b*, 65–66; compare *1905a*, 46–47. $\mathfrak{W}$ was proposed by Schönflies in *1900a*, 40–41.

similar (cardinally) to $\mathfrak{B}$; and make $u \varepsilon \mathrm{Cls}$ consist a hypothesis to the definitions of Nc'u, No'u,.. Thus e.g.,

$$u \varepsilon \mathrm{Cls}\ \mathrm{consist} . \supset : \mathrm{Nc}'u = \mathrm{Cls} \cap \upsilon \varepsilon (u \ \mathrm{sim}\ v) \quad \mathrm{Df.}$$

My reason for thus limiting the range of classes u for which (e.g.) Nc'u can be defined is that we get a contradiction if we assume that Nc'W exists ; i.e., Burali-Forti's contradiction *merely* depends on the *existence* of Nc'W (or No'W) for I think my proof in § 3 that W *is* well-ordered, is valid. Of course it now follows at once that every transfinite cardinal is an Aleph.

Cantor's conception of a consistent aggregate is not a logical definition ; it is ' an *absolutely* infinite Vielheit, which cannot be thought of as a One '.[1] I do not wish to insist upon the point, but it appears to me that the ' absolutely infinite ' (no cardinal) series of Alephs stand in an analogous relation to the *transfinite* cardinals (= Alephs) to that in which $\aleph_0$ stands to the *finite* cardinals. (Every finite cardinal is the cardinal of a segment of an ω and inversely ; every cardinal is the cardinal of a segment of the series of Alephs and inversely.)

If you think this limitation justifiable, will you let Mr. Whitehead see this letter ? Hardy objects that W is contradictory whether we talk of its cardinal or not. I think that W is not contradictory, but the class of classes similar to W (Nc'W) is.

Further I venture to think that not only Burali-Forti's (ordinal) contradiction does not occur now, but also your (cardinal) contradiction does not.[2] Please tell me if this is so.

Cantor had used the terms ' consistent ' and ' inconsistent ' in his limitation-of-size theory of the late 1890s to solve his paradox of the greatest cardinal, and as with Jourdain, his aim was to restrict his set theory to consistent ' multiplicities ' (*Vielheiten*). But he had developed his theory only in correspondence with Dedekind,[3] and when he corresponded with Jourdain he learnt that Jourdain had taken the term from E. Schröder.[4] Schröder used the word ' consistent ' to characterise a class of ' mutually compatible ' objects (*1890a*, 212,

[1] This quotation loosely paraphrases a passage in Cantor's letter to Jourdain of 4 November 1903, now published in my *1971a*, 116. But Jourdain's assertion contains a slip, for Cantor conceived of an " *in*consistent " class in this way.

[2] Another slip by Jourdain. The ' cardinal contradiction ' is Cantor's, not Russell's.

[3] That is, as far as any surviving Cantorian manuscripts are concerned ; see also Hilbert *1900a*, 184. The mathematical parts of this correspondence are published in Cantor *Papers*, 443–451. The texts were badly edited for publication, as described in detail in my *1974b*, art. 7.

[4] See my *1971a*, 117. Jourdain made the point himself in *1904b*, 67, and in *1905a*, 54–55. Thiel is in error in *1972a*, 138.

249); although it was not connected with paradoxes (which were unknown in 1890, at the time of his writing) it did lead him to a type theory, in which one type was used at a time (see my *1975a*, sect. 7).

We shall use double quotation marks when referring to Jourdain's theory of " consistent " and " inconsistent " classes. The status of both W and $\mathfrak{W}$ seem unclear. *Is* there a class $\mathfrak{W}$ with the property stated ? If so, is it unique ?[1] And can we deny to W a cardinal when it is claimed to be well-ordered ? Russell touched on this last point in his letter of 12 April, objecting to this attempted limitation on classes on the grounds that it

> seems to me merely a proposal not to talk about the awkward cases. A definition under a hypothesis is, to my mind, always objectionable, because propositions with hypotheses ought to hold when the hypothesis is false, whereas definitions under a hypothesis give mere nonsense if the hypothesis fails. And it seems to me plain that, whatever sort of class u may be, $v\ni(v \operatorname{sim} u)$ is a good class; thus by refusing to call this a number, we get out of no difficulties.

He went on to describe an attempt by Whitehead to avoid Cantor's paradox by a method which never reached a publishable form :

> Whitehead solves Cantor's difficulty about there being no greatest cardinal by the fact that, apparently,
>
> $$\alpha\varepsilon\mathrm{Nc} \,.\, \beta\varepsilon\mathrm{Nc} \,.\, \sim\supset \,.\, \alpha+\beta\varepsilon\mathrm{Nc}.$$[2]
>
> Cantor always assumes that the sum of two numbers is a number; but there seems no reason to believe this. I think Whitehead has proved
>
> $$\exists(\alpha+\beta) \,.\, \supset \,.\, \alpha+\beta\varepsilon\mathrm{Nc}$$
>
> but in the case of α's being the greatest cardinal, it is not certain that $\exists(\alpha+\alpha)$.[3] We have not yet reached Burali-Forti's puzzle, but I dare say it would disappear with sufficient care.

These passages provoked a lengthy exchange with Jourdain, to which we devote our next section.

[1] Russell made this point himself on p. 45 below. On ff. 59-61 of a large notebook now in the Russell Archives he made notes on Jourdain's *1904b*, *1904c* and *1905a*, and explicitly rejected Jourdain's solution of the paradoxes in *1904b*.

[2] ' $\sim\supset$ ' is Russell's rather unusual way of symbolising ' does not imply '. He similarly wrote ' $\sim\varepsilon$ ' for ' is not a member of ' several times in this correspondence, and ' $\sim=$ ' for ' does not equal '.

[3] For Whitehead's previous treatment of cardinal addition, see *1902a* (esp. p. 384) and *1904a*. See also Zermelo *1901a*.

8. *April–June 1904 : Russell's search for a comprehension principle*

Jourdain's reply came in the form of two letters, one of 14 April from Yateley and the other written on 13 May from his new address at Broadwindsor Manor, Beaminster, Dorset. In between, Russell visited Jourdain at Broadwindsor on 22 April,[1] but there is no record of their conversations. Thus we must rely on Jourdain's two letters (which we consider together) and Russell's answer of 15 May from Trinity College.

Let us take first Russell's rejection of definitions under hypothesis, for several of the other points can be related to it. He explained his views in greater detail :

> Definitions under a hypothesis are to my mind always objectionable, and both Whitehead and I now avoid them wholly. Suppose e.g. that you have only defined $a+b$ when a and b are numbers, and that you wish to consider subtraction. You want to put
>
> $$c-a=\imath x\{a+x=c\} \quad \text{Df.}$$
>
> If you have only defined $a+x$ when x is a number, it may be that e.g.
>
> $$a+\text{Julius Caesar}=c,$$
>
> and so you can't be sure that subtraction is unambiguous. And generally, it is an essential principle of right symbolism that every single letter occurring in a proposition must be able to take any value without making the proposition meaningless. For if the proposition becomes meaningless in certain cases, we can't state the fact without assuming that some sort of meaning does still attach to the proposition in those cases. All this is set forth conclusively by Frege.[2]

In *The Principles* Russell stated that definitions in philosophy formed part of the process of reducing complexes to basic indefinable components, whereas definitions in mathematics could only be of the nominal form.[3] But in this passage (and also in its predecessor) on the rejection of definition under hypothesis he anticipated a viewpoint which is urged nowadays as part of ' free logic ' (where assumptions of

[1] According to a mention in Russell's personal diary (see p. 3 above).

[2] Russell probably had in mind Frege's *1903a*, arts. 56–67. Frege made the same points later in *1906a*, esp. pp. 302–306 (*1971a*, 60–65).

[3] Russell *1903a*, art. 31 ; see also art. 108, and p. 15 above. In *Principia Mathematica* Russell forbad impredicative definitions because of the vicious circle principle (vol. 1, 36–37, though compare p. 45).

hypotheses within theory are avoided).[1] Russell's attitude to such definitions was either to arrange them so that if the hypothesis were not satisfied then the empty class was defined, or to leave the hypothesis out but bring it in later whenever needed. He mentioned this second approach in his letter of 15 May in response to a specific criticism from Jourdain; that when Whitehead had defined the multiplicative class $d^{\times}$ of a class d of classes, he had used the hypothesis that these classes be mutually exclusive:[2]

> In the case of $d^{\times}$, it is best simply to drop the hypothesis $d \varepsilon \mathrm{Cls}^2$ Excl, while of course retaining it when it occurs in propositions. But observe that *Transp.* e.g. is false when you have definition under hypothesis.[3] If $d^{\times}$ is defined under the hypothesis $d \varepsilon \mathrm{Cls}^2$ Excl, and if you have
>
> $$d \varepsilon \mathrm{Cls}^2 \mathrm{Excl} \,.\, \supset \,.\, f(d^{\times})$$
>
> you can't infer
>
> $$\sim f(d^{\times}) \,.\, \supset \,.\, \sim (d \varepsilon \mathrm{Cls}^2 \mathrm{Excl})$$
>
> because in the case supposed $\sim f(d^{\times})$ is meaningless. But this is intolerable.

However, while hypotheses may be rejected for definitions, they are required in order to save general theories from paradox. Jourdain made just such a point in his first letter to Russell, in defence of his theory of "inconsistent" classes against Russell's earlier criticisms:

> I think you are too severe when you say: 'It seems to me merely a proposal not to talk about the awkward cases'. For the proposal is not made to avoid an awkwardness, but is, it seems to me, *necessitated* by the fact that these awkward cases are *impossible* cases (i.e., lead of necessity to a contradiction).

Russell retracted somewhat in his reply, and then tried to reformulate the problem in terms of his current attempt to impose limitations on the definability of classes:

> I think I was perhaps too critical of your theory of inconsistent classes. But the point is that, if difficulties are to be avoided, we

[1] On aspects of free logic, see the collections of papers Lambert *1969a* and *1970a*; and on related problems of definitions, see Corcoran *1973a*, 40–46.

[2] Whitehead *1903a*, 383, ✳6.0. According to Russell *1902a*, 18, ✳2.34, Whitehead devised the definition of the class of mutually exclusive classes.

[3] *Transp.* is the analogue in logic of the *modus tollens* rule of inference (*PM*, ✳2.03 and ✳2.15–✳2.17):

$$p \supset q \,.\, \supset \,.\, \sim q \supset \sim p.$$

must say that, in the odd cases, only the functions exist, and there are no classes, whether consistent or inconsistent. We may give a purely conventional meaning to $x\varepsilon(\phi x)$ in cases when ϕx is queer, but if we do we shall not have

$$x\varepsilon(\phi x) = x\varepsilon(\psi x) \,.\, \supset : \phi x \,.\, \equiv_x \,.\, \psi x$$

because our conventional meaning will have to be the same in all the queer cases, or at any rate in a large proportion of them—e.g. we may decide that it is to be always $(p) \,.\, p$; but such decisions are pure conventions.

It is noteworthy to see Russell consider conventional meanings, and to use propositional quantification in assigning as meaning the falsehood '$(p) \,.\, p$'.[1] But this was not his main method of attack, and in much of this letter of 15 May he had much to say about the limits of definability. The motivation was a remark in Jourdain's second letter that ' neither in my notes of your 1901 lectures nor in Mr. Whitehead's paper [presumably *1902a*] can I find a proof of

$$u\varepsilon\mathrm{Cls} \,.\, \exists u \,.\, \supset \,.\, \exists \mathrm{Nc}`u'. \tag{8.1}$$

Jourdain also queried the statement in *The Principles*, art. 284 that ' since similarity is reflexive for classes, then every class has a cardinal number ' (a view expressed by Russell in another form on page 29 above), for in view of the paradoxes he doubted its truth. Russell discussed Jourdain's (8.1) on the " existence " of $\mathrm{Nc}`u$ as follows :

> There is nothing in my book or in my lectures bearing on the question which I gather you are interested in, but which is not really the question whether $\exists \mathrm{Nc}`u$. What you want to know, it seems to me, is whether
>
> $$\exists \mathrm{Cls} \cap v\varepsilon\{w\varepsilon v \,.\, \equiv_w \,.\, w \operatorname{sim} u\}.$$
>
> I assumed formerly that every function defines a class, and that therefore the above does not need to be specially stated for each function in turn. *I.e.* the symbol $x\varepsilon(\phi x)$ is always to be significant, and
>
> $$\phi x \equiv_x \psi x \,.\, \equiv_{\phi,\,\psi} \,.\, x\varepsilon(\phi x) = x\varepsilon(\psi x).$$
>
> If one assumes all this, then
>
> $$u\varepsilon\mathrm{Cls} \,.\, \supset_u \,.\, u \operatorname{sim} u : \supset : u\varepsilon\mathrm{Cls} \,.\, \supset_u \,.\, \exists \mathrm{Nc}`u.$$

[1] In *1906b*, 193, Russell interpreted '$(p) \,.\, p$' as ' everything is true '. His views on propositional quantification are discussed in more detail on pp. 92–93 below.

For $\exists \mathrm{Nc}'u$ means that $x \varepsilon \mathrm{Nc}'u$ is not false for all values of x; and $x \varepsilon \mathrm{Nc}'u$ is satisfied by putting $x = u$.

But I am now no longer prepared to say that *all* functions determine classes. I do not know, however, how to distinguish those that do from those that don't. I am working at this very point at present, but I have not as yet reached any conclusions. The question you ask is really whether $w \operatorname{sim} u$ (for constant u) is always a function which determines a class. My belief is that there are values of u for which $w \operatorname{sim} u$ does not determine a class; but I don't know how to determine what values they are. Or rather, there are functions which don't determine classes, and which, therefore, though satisfied by certain values of the variable, are not satisfied by any definable number of values: such is the function $x \sim \varepsilon x$. But when u is a genuine class, it seems likely that $w \ni (w \operatorname{sim} u)$ is also a genuine class.—The question of ordinals is subsequent to this question, which is fundamental in mathematical reasoning.

Later in the letter Russell contrasted the existence of a class, as determined by a propositional function, with its "existence" ('$\exists$'), whether so determined or not:

As to $v \ni (v \operatorname{sim} u)$ being always a class, I now admit that this may be doubted. But it is important to distinguish this from $\exists \{v \ni (v \operatorname{sim} u)\}$. E.g. $x \ni (x \sim = x)$ is a perfectly good class, namely Λ; but $\exists x \ni (x \sim = x)$ is false. The way I deal with the matter is to put

$$\mathrm{Reduc}\,(\phi x) \,.\, = : (\exists u) : x \varepsilon u \,.\, \equiv_x .\, \phi x \quad \mathrm{Df}$$

where

$$(\exists u) \,.\, \psi u \,.\, = \,.\, \sim \{\psi u \,.\, \supset_u .\, (p) \,.\, p\} \quad \mathrm{Df}^1$$

and *Reduc.* stands for ***reducible.*** We then require axioms as to the kinds of ϕ that are reducible. Don't confound

$$(\exists u) : x \varepsilon u \,.\, \equiv_x .\, \phi x$$

with

$$\exists u \ni (x \varepsilon u \,.\, \equiv_x .\, \phi x).$$

The latter involves the assumption that $x \varepsilon u \,.\, \equiv_x .\, \phi x$ is reducible, which the former does not. We have

$$(\exists x) \,.\, x \sim \varepsilon x, \quad \sim \exists \hat{x} (x \sim \varepsilon x).$$

[1] Another appearance of propositional quantification, but in this case only to supply the falsehood needed to complete the definition of existential in terms of universal quantification. Note that Russell used both Peano's '$\ni$' and Frege's apostrophe in this passage to denote class abstraction. His definition of '$\mathrm{Reduc}\,(\phi x)$' surely defines '$\mathrm{Reduc}\,(\phi \hat{x})$', and uses '$\exists u$' in the sense of existential quantification.

At least, whether we have the latter or not depends upon what conventional meaning we give to $\dot{x}(x \sim \epsilon x)$.[1]

In his reply of 24 May 1904 Jourdain pursued the question of the legitimacy of $w\ni(v \text{ sim } u)$, for it impinged upon his theory of " consistent " classes, which he described in further detail :

Do you think that Burali-Forti's contradiction will allow us (partly) to answer the question : Is w sim u (for constant u) always a function which determines a class ? by : Not if any part of u is similar to the aggregate W of all ordinals (or the field W of the generating relation of the series of all ordinals, arranged in order of magnitude) ? But, of course, since this criterion should be prior to all talk of ordinals, we must define an aggregate similar to W in another way. I thought Schoenflies had done this, but his statement[2] is wrong.

Then, if we could define such an aggregate, I would say, if $w\ni(w \text{ sim } u)$ is not a class, that u is an ' inconsistent ' class (my terminology is bad in every respect). Thus, Burali-Forti's contradiction would be solved[3] by $W \in \text{Cls}$ inconsist, which $\equiv \text{Nc}'W$ is not a class.

But there is, it seems to me, another way (which I prefer, because we must, for formal purposes, admit the notion of the class of *all* classes (Cls) while $\text{Nc}'W$ is a class of *some* classes) of dealing with Burali-Forti's contradiction. It is, in short, if u is a class, $w\ni(w \text{ sim } u)$ is also a class, and even $\exists \text{Cls} \cap w\ni(w \text{ sim } u)$; *but* in spite of this, $w\ni(w \text{ sim } u)$ may be self-contradictory. I will try to justify this. Firstly, it arises as follows :

From the existence of all the ordinals and Alephs (*The Principles*, art. 300) follows [the] existence of the type and cardinal of the series of all ordinals (or Alephs) arranged in order of magnitude. On the other hand, this series cannot have a cardinal or type, for it would be (respectively) an Aleph or an ordinal, and give rise to

[1] Note the use of ' $\exists$ ' here both for existential quantification and for " existence ". Jourdain referred to this part of Russell's letter in *1906b*, 268. In its final lines Russell thanked Jourdain for sending him a spare copy of F. Bernstein's dissertation *1901a* : ' I hadn't read it before, and I found it very interesting '.

[2] Jourdain now cited two passages in Schönflies's *1900a*, 40–41 about the class $\mathfrak{W}$. Schönflies claimed that the class W of all ordinals was (ordinally) similar to $\mathfrak{W}$, but Jourdain thought that it was similar only to a segment of $\mathfrak{W}$ (see Jourdain's *1905a*, 51–52). In two offprints of Jourdain's *1904b* that I have seen, Jourdain modified the definition of $\mathfrak{W}$ on p. 67 to read : ' . . . (well-ordered) aggregate such that every well-ordered aggregate is similar to it or to a segment of it, which is ordinally similar to W '. However, the defining condition still seems inconsistent.

[3] ' Solved ' is the best that I can do here with Jourdain's handwriting.

Burali-Forti's contradiction. So I concluded that though the cardinal ' exists ' in your sense, it is a self-contradictory class.

The explanation of this apparent contradiction seems to me to lie in that $\exists \mathrm{Nc}`u$ means that we can always find, when $u \varepsilon \mathrm{Cls}$, *a* class of classes similar to $u$; namely a class consisting of the class $u$, at least; while to prove that $\mathrm{Nc}`u$ is a class free from contradiction, we must find the class (there is evidently only one at most) of *all* the classes similar to u. In symbols, this comes as you suggest, to the proof that

$$\exists \mathrm{Cls} \cap \{v \ni (w \varepsilon v . \equiv_w . w \text{ sim } u)\};$$

i.e., if we put

$$N`u = v \ni (w \varepsilon v . \equiv_w . w \text{ sim } u) \quad \mathrm{Df},$$

that $v \varepsilon N`u$ is not false for all values of v.

This seems permissible, but one has to conclude that any ordinal or Aleph is not yet proved free from contradiction.

When you can spare the time, please tell me whether it is really permissible. Burali-Forti's argument then proves:

$$u, v \varepsilon \mathrm{Cls} . v \text{ sim } W : \supset . \sim \exists N`u.$$

These remarks help to clarify the reasons why Jourdain thought that a class u could exist but take no cardinal or ordinal. Following Russell's definitions, these numbers were defined as the class of classes (ordinally) similar to u, but they would not be definable at all if u were " inconsistent ". However, there may be particular classes which are (ordinally) similar to such a u, so that $\exists \mathrm{Nc}`u$ (or $\exists \mathrm{No}`u$). In other words, if u is " inconsistent " then $\mathrm{Nc}`u$ (or $\mathrm{No}`u$) will not exist though they might " exist ". We have seen reason on pages 28–29 above to doubt the legitimacy of the class $\mathfrak{M}$ in terms of which " consistency " and " inconsistency " are defined; it now appears that an element of conflation of existence with " existence " may also be involved in Jourdain's theory.

In his reply of 4 June from Tilford, Surrey, Russell raised a more fundamental point about Jourdain's theory. For he expressed his doubts that the paradoxes were caused at all by excessively large classes:

> I feel it quite impossible to know what one ought to say about Burali-Forti's Contradiction until the other is settled. It is a mistake to suppose that the ' inconsistent classes ', or, as I prefer, ' irreducible functions ', are the very large ones. They are only half way up. For the negation of a reducible function is reducible.[1]

[1] Then, by *modus tollens*, if $\phi \hat{x}$ is irreducible then so is $(\sim \phi \hat{x})$.

We have (writing $\phi\hat{x}$ for the function itself, as opposed to any of its values, which we represent by ϕx) :[1]

$$\sim \text{Reduc}\,(\phi\hat{x})\ .\ \equiv\ :\ \sim(\exists u) : \phi x\ .\ \equiv_x .\ x \varepsilon u.$$

This requires that, given any u, either ϕx is sometimes true when $x \varepsilon u$ is false, or ϕx is sometimes false when $x \varepsilon u$ is true. Thus it is just as much the terms *not* satisfying ϕx as the terms satisfying it, that make it irreducible.

My own feeling is that it is good to put a definition of this sort :

$$\text{Elem} = u \ni (\phi x\ .\ \supset_x .\ x \varepsilon u : \supset\ .\ \text{Reduc}\,(\phi\hat{x})) \quad \text{Df} \qquad [(8.2)]$$

where *Elem* stands for Elementary. We have

$$u \varepsilon \text{Elem}\ .\ \supset\ .\ 2^{\text{Nc}'u} > \text{Nc}'u$$

and I think the converse implication also holds. I *think* it can be proved that the field of a well-ordered relation is always elementary ; hence the Arithmetic of the Alephs will proceed as though all functions were reducible, except when it comes to considering the whole series of Alephs. I do not know whether it can be proved that the generating relation of the whole series of Alephs is reducible. (The theory of the reducibility of relations, i.e. functions of two variables, is analogous to that for functions of one variable, but it is complicated. I have not yet got it into shape.)

I hold that $w \ni (w \text{ sim } u)$ is always a proper class when u is so. This I hold on certain general grounds, but I dare say it is not true.

The closing paragraphs show some advance in that the elementarity of classes is related to their satisfaction of Cantor's theorem that the cardinal of a class is less than that of its power-class.[2] But the defining condition (8.2) seems ill-formed in containing 'ϕ' free (presumably a universal quantifier for 'ϕ' is required), and since it relies anyway on the unspecified property of reducibility of the corresponding propositional function then the role of the power-class theorem can only be limited. By contrast, Zermelo had a power-class axiom in his system (*1908b*, axiom IV), but as part of a *comprehensive* axiomatisation of set theory.

[1] Note the explanation of the circumflex to denote functional abstraction—possibly an indication of its recent adoption by Russell. When reprinting Russell's *1908a*, 247 in *1956a*, 85, the editor, R. C. Marsh, omitted the circumflex from Russell's first published explanation of the symbol !

[2] Cantor *1891a*. Compare Russell *1903a*, arts. 345–349, the undated manuscripts 'Proof that no function takes all values' and 'No greatest cardinal', and the worksheets 'FN', esp. ff. 603–619. See also Cantor's analysis of Russell's paradox, described to Jourdain in my *1971a*, 119 and reported by Jourdain in his *1905a*, 55.

In this letter of 4 June Russell also described his current doubts of Whitehead's non-additivity theorem for transfinite cardinals which he had reported with confidence the previous April (see page 29 above):

> The proposition
>
> $$\alpha, \beta \varepsilon \mathrm{Nc} \,.\, \sim \supset \,.\, \alpha + \beta \varepsilon \mathrm{Nc}$$
>
> is not yet well-established, and I have my doubts about it. The definition of $\alpha + \beta$ is
>
> $$\alpha + \beta = w \ni (\exists (u, v) \ni \{u \varepsilon \alpha \,.\, v \varepsilon \beta \,.\, u \cap v = \Lambda \,.\, u \cup v = w\}) \quad \text{Df.}^1$$
>
> The way in which it seemed as though $\alpha + \beta \varepsilon \mathrm{Nc}$ might fail was that if α was the greatest cardinal, it might happen that
>
> $$u \varepsilon \alpha \,.\, v \varepsilon \alpha \,.\, u \cap v = \Lambda$$
>
> would be impossible; whence
>
> $$\alpha + \alpha = \Lambda.$$
>
> But I think it can be proved that this does not occur.

Again, the argument is too lacking in clear criteria for precise evaluation. If the universal class V really is universal, then under the above definition $(\alpha + \alpha)$ will certainly be empty. If, however, arithmetic were defined in a way which did not associate its greatest cardinal with the limit of definability of classes, then $(\alpha + \alpha)$ might be non-empty. There was no way of determining the status of $(\alpha + \alpha)$ until the criteria of definability were established; but $(\alpha + \alpha)$ was being used as a means of finding those criteria. Russell was going round in circles, of which some were vicious. He described his predicament in this letter with regard to the progress of *Principia Mathematica*:

> I fear it will be a long time—three years at least—before Vol. II appears. There is a frightful lot of work to be done, and also I hope to put in a proper solution of the Contradiction.

9. *July–December 1904: transfinite ordinals, and continuity*

Jourdain somewhat changed the line of the discussions in his next letter, which was written on 14 July. His continuing interest in Burali-Forti's paradox of the greatest ordinal led him to try to improve his

[1] Compare the definition given by Russell in Whitehead *1902a*, 379, ✳1.92, especially the use there of the hypothesis that α and β be cardinals, and the *non*-use of ordered pairs.

theory of " inconsistent " classes by considering Cantor's method of constructing transfinite numbers described in *1883b*, arts. 11–13.

Cantor used three ' principles of generation ' (*Erzeugungsprincipien*) in the construction of ordinals. The first was the successor operation of (assuming 1 and) adding 1 onto any ordinal already created. The second postulated the existence of a limit ordinal at the end of a well-ordered series of ordinals which had no final member. The principles worked in tandem, constructing a series of ordinals by an alternation of iterated succession and postulation of a limit ordinal to serve as the first member of the next series.

Cantor's further awareness of different orders of transfinite cardinality caused him to adjoin a third principle, the ' principle of limitation ' (*Hemmungsprincip*), which split up the ordinals into number-classes by the criterion of the cardinality of its segments. The finite ordinals, denumerable in number, formed the first number-class, and their cardinality—the smallest infinitude possible—was assigned the symbol ' $\aleph_0$ '. (Cantor did not use the aleph notation in his *1883b*.) The second number-class was the class of transfinite ordinals η for which the segment from 1 to η was of cardinality $\aleph_0$. Its initial ordinal was ω, and its cardinality was proved to be greater than $\aleph_0$ and assigned the symbol ' $\aleph_1$ '. The third number-class was defined similarly in terms of segments of cardinality $\aleph_1$, and had ω_1 as initial ordinal ; and so on.

Jourdain's argument began with the following version of Cantor's third principle :

> Let α be any ordinal, then either α is the first number of a number-class ($\alpha = \omega_\nu$),[1] or there is a first number (ω_ν) after α which is the first of a number-class. In both cases we call ω_ν the ' class-characteristic ' of α.
>
> Then the principle in question states : The cardinal of all the ordinals < the class-characteristic ω_ν of any ordinal is $\aleph_\nu$. (1)
>
> Now, let β be the type (an ordinal) of the series of Cantor's ordinals ; the cardinal of this series must be $\aleph_\beta$ (every Aleph less than it is the cardinal of a segment of the β), and since β is its own class-characteristic (in fact, every predecessor belongs to a lower class than some other number between it and β), the condition (1) is not satisfied ; and so β is not the greatest Cantor's ordinal but the least ordinal > all Cantor's ordinals and is not a Cantor's ordinal (does not satisfy (1)). Herein is no contradiction.

[1] Jourdain now cited Russell *1903a*, art. 300. The notation ' ω_ν ' for the initial ordinal of the $(\nu+2)$-th number-class was used by Cantor for small ν in his correspondence with Dedekind (Cantor *Papers*, 446), but Russell seems to have introduced it independently (see Jourdain *1904c*, 295). Cantor used the notation otherwise in *1897a*, art. 20, thm. B.

But of course Burali-Forti's contradiction subsists for *all* ordinals (including those not subject to Cantor's restrictions).

Given any well-ordered series of ordinals with or without a maximum, there is *always* a next greater than all of these. This '*absolutely*' infinite series can have no type, for if it had, say β, there would be an ordinal β not in the series but the next > all its terms, contrary to the supposition that the next to *any* [sic] series of ordinals is in the whole series of ordinals.

This is the way I now think of Burali-Forti's contradiction. If a series has a type, it seems to have a 'gewisser Abschluss' (Cantor *1883b*, 34) ['a certain conclusion'; *Papers*, 196]. The series of all ordinals is then *absolutely* infinite (≡'inconsistent'), though Cantor's ordinals are not so, in spite of what he says.[1]

I hope my remark is of some little interest.

Jourdain published soon afterwards an extension of this argument in his *1905a*, 51–56 (see also his *1905g*, 466, and *1906b*, 270–272). He analysed the series S of ordinals definable by means of Cantor's three principles of generation. Assume that S has the ordinal β. Then β is the first ordinal beyond the scope of the number-class construction, and so must be an initial ordinal; for otherwise it would have predecessors, which would belong to a Cantorian number-class and imply that β belonged also. Thus we have

$$\beta = \omega_\alpha, \tag{9.1}$$

where ω_α is the initial ordinal of some (presumably non-Cantorian) number-class.

Now S is ordinally similar to the series I of transfinite initial ordinals (or class-characteristics):

$$\omega, \omega_1, \omega_2, \ldots \omega_\omega, \ldots . \tag{9.2}$$

But (9.1) shows that β belongs to I, whereas it is larger than each member of I. Hence ω_α exceeds the members of I in the same way that β exceeds each member of S, so that

$$\beta = \alpha. \tag{9.3}$$

Hence (9.1) may be re-written:

$$\omega_\beta = \beta. \tag{9.4}$$

Now if β were a Cantorian ordinal, then (9.4) could not hold.

[1] Jourdain now cited Cantor *1883b*, 44 (*Papers*, 205). Presumably he had in mind Cantor's statement that 'The absolutely infinite series of numbers thus appears to me in a certain sense as an appropriate symbol of the absolute'.

Further, the cardinal of β should be less than $\aleph_\beta$ (Jourdain seemed to assume this), whereas it equalled $\aleph_\beta$. Hence β is a non-Cantorian ordinal; in fact, it is the order-type of W, the class of 'all' ordinals without restriction to number-classes.[1] Thus (for Jourdain) the way to conceive of "consistent" and "inconsistent" classes was to accept the existence of W by Cantor's first two principles, but because of Burali-Forti's paradox to assign to it the status of "inconsistency" and deny both its order-type β and also (9.4).

To some extent Jourdain's new, confused, argument resembles contemporary researches by G. Hessenberg (*1906a*, art. 85) and especially F. Hausdorff (*1908a*, 442–444), in which (9.4) was not used as a criterion for "inconsistency" but as a source of generating very large ordinals. Jourdain was to discover more of Hausdorff's theory for himself later, and so we shall postpone its description until pages 97–98 below. But as it stands his argument seems to embody dubious assumptions (especially concerning β) even without the benefit of Hausdorff's hindsight. Russell might have been expected to be critical, but in fact in his reply of 20 July 1904 from Tilford he was non-committal :

> Many thanks for your interesting letter. I have always supposed Burali-Forti was considering *all* ordinals, without subjecting them to Cantor's Hemmungsprincip. I forget whether he says anything to that effect or not. You will see from my article in *Revista di Matematica* [*1902a*, 43] that I take Burali-Forti as considering *all* ordinals. But I am much interested to see exactly, from your letter, the bearing of the Hemmungsprincip on the problem.
>
> I do not yet indulge in any opinion about the solution of Burali-Forti's difficulty : I find from experience that so many possibilities of error exist that one has to go on with the strictly symbolic development bit by bit, without any decided view as to parts not yet tested. I do not, of course, mean that everyone who works at the subject ought to do this, but that for Whitehead and me, in our Vol. II, it is quite unavoidable at our present stage, in which correctness rather than new creation is our aim.

The correspondence now lingered for some months. On 12 October 1904 Russell passed on a report from L. Couturat that at the International Congress of Mathematicians held the previous month at Heidelberg

[1] As Jourdain pointed out in his *1905a*, 51, the purpose of the third principle was sometimes misunderstood at that time; it splits up the series W produced by the first two principles alone.

a certain König of Budapest gave a proof that the continuum cannot be well-ordered, and that Cantor, who was present, was unable, at least at the moment, to find any flaw in the demonstration.[1] I have not seen his proof, so I can form no opinion on the matter. I presume he will shortly publish his work.

Unfortunately there is no trace of Jourdain's reaction to this news, which contradicted his (and Cantor's) belief in the provability of the well-ordering principle. Soon afterwards König withdrew this proof (*1905a*) and substituted another (*1905c*), which also proved to be defective. We shall look at them on pages 53 and 49 below respectively.

The next letter came from Jourdain on 28 November, and concerned Cantor's two definitions of continuity, as discussed by Russell in *The Principles*. With his analyses of the different kinds of order-type Cantor pioneered the study of ordinal properties of continuous series. In particular, he realised that continuity would have to be more carefully defined than the standard Leibnizian definition, of the intermediacy of members of a continuous series between any two given members—' everywhere dense ' in his language, ' compact ' in Russell's, or ' dense ' in modern terminology.[2] In his *1883b*, art. 10 he stated that two properties were needed to specify the order-type θ of the continuum P; ' perfection ' and ' connectedness '. Perfection is the property that P should equal the class P' of its own accumulation points. Connectedness is the property of having no finite gaps, and depends on the geometrical notion of distance in its precise definition. To Cantor this dependence was a defect, and in his *1895a*, art. 11 he replaced connectedness by the purely ordinal property that P contains between any two of its members at least one member of a rational sub-ordering η (itself redefined ordinally in art. 9).

In his letter Jourdain queried Russell's claim in art. 277 of *The Principles* that Cantor's second definition ' includes precisely the same class of series as those that were included ' in his former definition. His query was based on examples constructed in A. Harnack *1884a* and R. Bettazzi *1888a* of continuous series which nowhere are everywhere dense. To him they ' seem to show that the concept θ is wider than

[1] Couturat's letter (in the Russell Archives) is dated 5 September 1904, and shows that Couturat received the information from H. Fehr (compare Fehr *1904a*, 385). Cantor's behaviour (described in Kowalewski *1950a*, 198–205) shortly preceded one of his attacks of mental illness (see my *1971b*, 370).

[2] In the first part of *The Principles* Russell used the word ' continuous ' to denote this property, but in art. 259 (see also art. 185) he assigned ' compact ' to it and identified it with Cantor's ' everywhere dense ' as defined in Cantor's *1895a*, art. 9. Cantor gave a different definition earlier in *1879a*, 2–3 (*Papers*, 140). ' Dense ' seems to be due to Hausdorff (especially in *1907b*, 541; compare *1914a*, 84).

one would suppose ', and might instil ' a doubt that ordinal continuity is sufficient ' for the specification of continuity by contravening the purely ordinal property used in Cantor's second definition.

Jourdain had to wait for nearly a month before Russell's reply came on 20 December 1904 from Whitehead's house at Granchester, near Cambridge :

> I have waited to answer your letter till I had a chance of talking it over with Whitehead, as I did not know quite what the answer was. There is great difficulty (more, I think, than I quite realised in writing my book) in comparing Cantor's two definitions of continuity, because the first involves distance, and at least *seems* to involve having another series in which the series concerned is contained. For otherwise, it is hard to give any meaning to the notion of gaps. A θ not contained in another series *is* dense in any interval. I find Borel gives an instance of such a θ as you mention, but, like all the instances given by ordinary mathematicians, it consists of a selection of the real numbers, so that it is conceived from the start as contained in another series.[1] Taken alone, it has all the ordinary properties of a θ. But it is obvious that you can, given a θ, so interpolate terms as to produce a new larger series in which your given θ is no longer condensed in every interval.[2] It is this surreptitious introduction of a larger series, which is involved in choosing series composed of real numbers, that leads to the difficulty. If a series is not regarded as contained in another, the ordinal θ has all the properties required of a θ;[3] and if you allow your series to be contained in another, without specifying that other, no amount of definition of your original series can tell you whether its limit-points belong to it or not, since that will depend upon the manner of interpolation of the new terms. The mere fact that θ, ordinally defined, is a *type*, as Cantor proved,[4] is enough to show that, so far as purely ordinal properties are concerned, nothing more special can be required.

In other words, one way of expressing the reliance of Cantor's first definition of continuity on geometrical considerations is to say that the definition presupposes a super-class, such as a geometrical continuum,

[1] Presumably a reference to Borel *1898a*, 39–46, which contains examples of perfect classes which are nowhere dense.

[2] Compare Russell's earlier remarks on intercalation on pp. 19–20 above.

[3] A rather confusing double use of ' θ '.

[4] Russell now cited Cantor's *1895a* and *1897a*, presumably in connection with Cantor's claim in *1895a*, art. 11 that these exist classes of order-type θ. Cantor had virtually no existence axioms or theorems in the later senses of these terms.

to contain the gaps which a connected class does not possess. By contrast, the ordinal definition of the rational order-type in the second definition showed no such dependence. Cantor had long been aware of such distinctions, for in his *1884a*, 472–473 (*Papers*, 229) he contrasted the geometrical character of density in an n-dimensional space G_n—the traditional definition of continuity—with denseness-in-itself, the property of a class that it is contained within the class of its own accumulation points :

> The expression '*dense-in-itself*' denotes *on its own* a specific property of a class ; on the other hand, '*everywhere dense*' does not have in itself the significance of a class-property, but becomes such only if one uses it in connection with a particular n-dimensional continuous portion H of G_n, when one can say of a class P, that it is '*everywhere dense in H*'.

10. *April 1905 : axioms of choice, and new paradoxes*

In the spring of 1905 Russell and his wife Alys moved to a new house at Bagley Wood on the south side of Oxford : a lovely location, at the time the only house in the area, with an extensive formal garden containing beautiful statues (see my *1974c*). For some years the correspondence with Jourdain passed between this house and Jourdain's home at Broadwindsor Manor.

The next letter was written by Jourdain on 25 April. He enclosed with it the manuscript of a paper containing 'an attempt to state my conclusion about Burali-Forti' for which 'the needful discussion may be the better provoked if it is published in a philosophical journal', and he asked Russell to recommend it for *Mind*. When Russell replied three days later, he enclosed two pages of detailed comments but advised as follows :

> Your article has much interested me, and I should be glad to see it published anywhere. But I am returning it to you, because I do not think *Mind* the best place. Few mathematicians read *Mind*, and of the readers of *Mind* very few would understand a word of what you say. Possibly *Nature* would print it. But in any case you should choose a journal read by mathematicians. I don't think the discussion would be considered excessive ; except *possibly* the historical sketch of Cantor's work. Still, if you prefer *Mind*, I will gladly send it to the Editor, with a note saying that I think it ought to be printed.

Before we examine Russell's comments, we shall describe the fate of Jourdain's manuscript. Jourdain modified it in accordance with Russell's suggestions, and in a letter of 6 May stated that he preferred that it go to *Mind*. Russell replied on 15 May : ' As you prefer *Mind*, I will glady send your manuscript to Stout [the editor] with a letter to say I think it important ; or I could give it to him on June 1, when he will be here. Only I fear few readers of *Mind* will understand you. In *Mind*, the historical introduction will of course be very appropriate'. Jourdain sent him the revised version on 17 May, and on 15 June Russell informed him that ' I sent your manuscript to *Mind* immediately with a letter saying I thought they ought to print it ; but I have not yet heard from them about it. Perhaps they have written to you '. However, Jourdain heard nothing, and on 31 July Russell wrote : ' I hope *Mind* will print your paper ; I have heard nothing about it, and Stout (the editor) is a bad correspondent, so probably you will not hear until you get proofs. *Mind* usually waits about nine months before it finds room for an article '. But the optimism was unjustified. Jourdain seems to have reported its rejection by *Mind* in October, for on 2 November Russell wrote : ' I am sorry about the article for " Mind ". It is a difficulty inherent in our sort of work. I have lots of things I have to keep for my second Volume, (though I should like to publish them separately) because there is no magazine that would publish them. Why don't you try Peano's Revue des mathématiques ? '

Jourdain did publish a paper (*1906c*) in Peano's journal, which seems similar in places to the one criticised by Russell and does show signs of comment from Russell. But neither it nor any of Jourdain's other papers seems identifiable as this manuscript, which we must therefore regard as a lost work, unable to provide us with the contexts of Russell's comments. However, the comments themselves and the accompanying letter indicate Russell's current state of mind on various matters.

We may take first his stress of the similarity between his paradox and the liar paradox :

> My Contradiction is not overcome by denying that every class has a cardinal number. It arises at an earlier stage, before cardinals have been introduced. It is more analogous to the old puzzle about the Cretan who said all Cretans were liars. Consider the following : David said at 2.30 that all his remarks between 2 and 3 were lies ; and all his other remarks during that hour were palpable lies. Was his remark at 2.30 a lie ? Either hypothesis implies its contradictory. (This form was suggested to me by a friend.)

Russell's view of the similarity between his and the liar paradoxes

shows his tendency to associate together the mathematical and the semantic paradoxes (as they are now called ; see pages 89–90 below).

Another remark shows that he did not include in his current efforts the possibility of denying that a class would be a member of itself or of another class :

> It is not from the assumption ' that a class can always be an element of another class ' that contradictions result. My contradiction is :
>
> $$w = \hat{x}\{(x \sim \varepsilon x) . \supset : x \varepsilon w . \equiv_x . x \sim \varepsilon x : \supset : w \varepsilon w . \equiv . w \sim \varepsilon w.$$
>
> You can't escape this by saying that w is not a member of any class ; for to say it is *not* a member of w is proved to be equivalent to saying it *is* a member of w.
>
> Why should not a class be one of the elements of itself ? I see no contradiction in this.

Russell missed a potentially fruitful approach here, for had he denied the principle that a class can belong to itself or to another class then he might have thought of considering classes that could not belong to any class, a kind of class first studied by J. von Neumann in his *1925a*.[1] However, it would be rash to expect that Russell could have anticipated von Neumann's theory to a substantial extent, for he never attempted a *full-scale* axiomatisation of set theory that von Neumann developed.

Jourdain must have mentioned in his paper the class $\mathfrak{W}$ defined on page 29 above by the property that each well-ordered class is ordinally similar to (a segment of) it, for Russell objected to Jourdain's talk of ' the ' class $\mathfrak{W}$:

> You ought to speak of *a* series such that etc. One has ' *the* series of ordinals in order of magnitude ' ; but your new series is not unique : e.g. it still fulfils your definition if any two of its elements are interchanged. Such points are very important, and errors in respect of them frequently lead to fallacies ; e.g. the ontological argument for the existence of God rests on a mistake of this kind. You do not show the existence-theorem in the present case, but by using *the* for *a* you conceal the need of it.[2]

[1] Cantor's analysis of Russell's paradox (see my *1971a*, 119) contains the seeds of such an approach.

[2] Jourdain referred to this criticism in *1906b*, 268, and *1906c*, 130, acknowledging the need to modify his remarks about ' the ' class in *1904b*, 67, and *1905a*, 51–53. On the use of ' the ' in the ontological proof of the existence of God, see Russell *1903a*, art. 107.

Another remark by Russell touched on the general problem of modifying both mathematics and logic in order to avoid the paradoxes :

> 'I think both you and other mathematicians do not realize how any limitation such as you propose involves a modification of the fundamentals of logic, since these, as commonly accepted, exclude any such limitation. When the right limitation has been found, it is likely to exclude more than might be wished. Meanwhile, suspense of judgement seems alone justifiable. But I do not deny anything you say on this subject ; I merely suspend judgement.

Russell also commented on three recent publications which Jourdain must have mentioned in his paper. The first was Zermelo's famous paper *1904a* in which he showed that if we assume an axiom of choice then Cantor's well-ordering principle can be proved. Russell commented :

> Zermelo's proof is a failure, since, putting
>
> $$\mathrm{Mult}'w = \dot{R}\{R \epsilon \mathrm{Nc} \to 1 \,.\, \mathrm{D}'R = \mathrm{Cls}'w - \iota'\Lambda :$$
>
> $$uRx \,.\, \supset_{u,x} \,.\, x \epsilon u\} \quad \mathrm{Df}^1$$
>
> he assumes
>
> $$w \epsilon \mathrm{Cls} \,.\, \supset \,.\, \exists'\mathrm{Mult}'w,$$
>
> which is doubtful. But he *does* prove
>
> $$w \epsilon \mathrm{Cls} \,.\, \exists'\mathrm{Mult}'w \,.\, \supset \,.\, w \epsilon C''\Omega$$
>
> which is an important and interesting theorem.[2]

It is interesting to see Russell describe Zermelo's proof as a ' failure ' because of its assumption of a mapping from the power-class of w to w in order to determine a selection function R. Such an attitude exemplifies the kind of controversy that was to surround axioms of choice for some years.

Russell called the class of selection functions R ' Mult'w ', an abbreviation of ' the multiplicative class of w ', because such functions and their classes had already arisen in Whitehead's treatment of the multiplication of cardinals. On page 31 above we saw mention of the multiplicative class $d^\times$ of a class d of mutually exclusive classes as an

[1] Russell noted in the margin that he now represented class abstraction by Frege's apostrophe rather than by Peano's ' ι '. (Cls'$w - \iota'\Lambda$) is the class of non-empty subclasses of w, a class symbolised as ' Cl ex'w ' in *PM*, ✳60.13.

[2] ' Ω ' is Russell's symbol for the class of well-ordered series (compare *PM*, ✳250.02). Thus, as he wrote in the margin here, ' $C''\Omega$ = fields of well-ordered relations '.

example of a definition under hypothesis. $d^{\times}$ was the class of choice classes of d, that is, of sub-classes of the union of d whose intersection with each member of d forms a unit class :

$$d \varepsilon \mathrm{Cls}^2 \text{ excl } . \supset : d^{\times} = \mathrm{Cls} \cap ms\{p \subset d \, . \supset_p . \, p \cap m \varepsilon 1 : m \subset \mathrm{U}`d\} \quad \text{Df.}^1(10.1)$$

It was called the ' multiplicative class ' of d because it was the class of classes by means of which the product of an infinity of cardinals could be defined. Its construction in 1902 is one of the unintended assumptions of an axiom of choice, called later by Russell the ' multiplicative ' axiom and asserting that $d^{\times}$ " exists ", that is, that there exists at least one choice class.

Another assumption of such an axiom was the subject of Russell's second comment to Jourdain about recent literature. Hardy had just published a paper containing the following construction of a class of cardinality $\aleph_1$. He associated with each ordinal of the first and second number-classes a denumerable ascending sequence of finite integers as follows. (1, 2, 3, . . .) corresponds with 1. If with an ordinal μ is associated $(b_1, b_2, b_3, \ldots)$ then with $(\mu + 1)$ is associated $(b_2, b_3, b_4, \ldots)$, while to a limit ordinal γ there corresponds the diagonal sequence taken from the denumerable series of sequences associated with the ordinals for which γ is the limit. The process of constructing the sequences is not unique since (not only does it depend on the sequence chosen for 1 but also) a limit ordinal is the limit of various sub-series of its predecessors ; for example, ω is the limit of (1, 3, 5, . . .) as well as of (1, 2, 3, . . .). But it can be shown that, for a given process of construction, each ordinal will have its distinct sequence ; that is, even while two sequences may have common members at first, eventually they will differ. Now there are $\aleph_1$ such ordinals, so that they form a class of cardinality $\aleph_1$, as desired (Hardy *1903a*, 89–93).

Hardy's construction was prefaced by a theorem concerning the relationship between the cardinality $2^{\aleph_0}$ of the continuum and the series of alephs (*1903a*, 88). Since $\aleph_1$ is the next aleph greater than $\aleph_0$ and $2^{\aleph_0} > \aleph_0$, then we can remove $\aleph_1$ members from a class C of cardinality $2^{\aleph_0}$; and if C is not exhausted, $2^{\aleph_0} \geqslant \aleph_1$. If in fact $2^{\aleph_0} > \aleph_1$, then we can remove $\aleph_2$ members from C to show that $2^{\aleph_0} \geqslant \aleph_2$, and so on. Hence $2^{\aleph_0}$ either equals an aleph or is greater than each of them.

Hardy's arguments contain several uses of infinite selections, both in the choice of sequences to associate with limit ordinals and also with the removal of members from C. However, Jourdain, who also tried

[1] Whitehead *1902a*, 383, ✳6.0 (with ' $\subset$ ' written instead of ' $\supset$ ' for improper inclusion) ; among other such assumptions in this section, see ✳2.76 and ✳4.3. See also Russell *1901c*, art. 4, ✳3 on definitions of infinity (probably an echo of Dedekind's preface to the second (1893) edition of *1888a*).

to construct classes of cardinality $\aleph_1$,[1] accepted Hardy's theorem and used it to lend support to his theory of " consistent " classes by regarding as a contradiction the possibility that a cardinal could be greater than any aleph (see his *1904b*, 63–65). But Russell was doubtful of the legitimacy of Hardy's theorem, and pointed out to Jourdain its use of infinite selections :

> I think Hardy's argument that an Nc is either equal to some Aleph or greater than all of them is fallacious. It would involve, if written out formally, the assumption of the existence of a certain multiplicative class, i.e. the assumption that, given a mutually exclusive class of classes, none of them null, it is possible to find at least one class composed of one term out of each class of the class of classes. A similar assumption is involved in Zermelo [*1904a*]. I have worked long at assumptions of this kind, without seeing any reason to think them true.[2]
>
> I agree now that the series of all ordinals is well-ordered.

The final sentence (which completes our main quotations from the notes)[3] is very striking, for it shows Russell's current acceptance of Jourdain's talk of ' the series of all ordinals '. He complemented this view in an opinion in the letter accompanying the notes on the third recent publication—F. Bernstein's attempt in his *1905a* to avoid Burali-Forti's paradox by defining the class W of all ordinals as the order-type of segments of well-ordered classes and then denying that W was itself such a segment. ' As if W was not a segment of (W, β) ', Jourdain remarked to Russell, in allusion to his (alleged) variant of Burali-Forti's paradox in terms of $\omega_\beta = \beta$,[4] and Russell at least agreed in spirit :

> Bernstein's article in *Mathematische Annalen* strikes me as rot.

[1] Jourdain sent one construction to Cantor in a letter, who replied that he found it incomprehensible although he understood Hardy's at once | (see my *1971a*, 118–119).

[2] Jourdain referred to this passage in *1906c*, 129. Very few of Russell's manuscripts of this time relate to the multiplicative axiom ; perhaps the most relevant are ff. 633–636 and 792–794 of the worksheets ' FN '.

[3] In four other brief comments, Russell mentioned that there were different ways of constructing classes, advised Jourdain to speak when appropriate of ordinal numbers and not just of numbers, recalled that Burali-Forti discovered the ordinal paradox, and recommended Jourdain to use ' similar ' rather than ' equivalent ' : ' I use *similar* for classes, *like* for relations ; i.e. (when D'S is the domain of S, F'P the field of P)

$$P \mathscr{L} Q \,.=.\, (\exists S) \,.\, S \varepsilon 1 \rightarrow 1 \,.\, \mathrm{D}`S = \mathrm{F}`P \,.\, Q = \breve{S}|P|S \quad \mathrm{Df}$$

where $\breve{S}|P|S$ stands for the relative product of $\breve{S}$, P and S. I find this usage convenient, and I think you also would find it so '.

[4] See pp. 39–40 above. Compare Zermelo's remarks on Jourdain's and Bernstein's proofs in *1908a*, art. 2.

I have at present no view on Burali-Forti's contradiction; but I am sure the solution lies in the foundations of logic, not in any late technical mathematics. To deny that W has a type or a cardinal may, for ought I know, be true; but since the definitions of its type and cardinal are, in form, just like hosts of other definitions, their exclusion requires some logical principle, and it is this principle that I am in search of.

Russell went on to point out the vastness of the objects which he defined as cardinals (a feature which has caused disquiet to some of his critics, such as Jourdain in *1907a*, 355):

Note that every cardinal except 0 is a class having as many members as there are entities altogether; e.g. if V is the class of all entities, we have $1 = \iota``V$, where

$$\phi``(u) = \hat{y}\{(\exists x) \,.\, x \epsilon u \,.\, y = \phi`x\} \quad \text{Df};$$

and $\iota``V$ is plainly similar to $V$. (The apostrophe in $\phi`x$ may be read *of*; its only purpose is to be able to double it in $\phi``u$.)

On page 26 above we found Russell to be surprisingly dubious about a proof by Jourdain that the class of finitely constructible real numbers was denumerable. By now he had noticed the version that Jourdain had published in *1905a*, 43–44, and in this letter he not only shed his doubts but even offered the following generalisation:

'The total number of *all* mathematically definable entities is Aleph_0'. For the number of indefinables is finite; and every definition is (symbolically) a finite series of indefinables, in which any one of them may be repeated a finite number of times; but the numbers of such series is Aleph_0. This has awkward consequences, e.g. that between ω and ω_1 there must be a smallest indefinable ordinal. I don't know how to avoid contradictions here.[1]

This statement shows Russell's awareness of paradoxes concerning indefinable ordinals. These paradoxes are usually associated with arguments due to J. Richard *1905a* (a consequence of his *1903a*, 107–113) and with König's second alleged proof *1905c* that the continuum could not be well-ordered. Both lines of reasoning hinged on the existence of indefinable objects; Richard's on the least indefinable ordinal, and König's on the complement of the class of finitely definable real numbers (whose first member could be so finitely definable if the continuum were well-ordered). But these papers—which we now recognise as concerning definability itself rather than the objects defined

[1] Jourdain referred to this passage in *1906b*, 277.

—were not written until around June 1905. Russell may have constructed the above argument from a result known since Russell as ' Berry's paradox '.

George Godfrey Berry (1867–1928) was a part-time employee at the Bodleian Library in Oxford, an occasional translater, and in Russell's opinion ' the only man I found in Oxford who knew any mathematical logic '. This statement comes from Russell's notes on Berry that accompany Berry's only known documentary contributions to logic—the letters that he wrote to Russell between 1904 and 1910.[1] A separate occasion is needed to describe the contents of these amazing letters, but relevant for us now is the first one, dated 21 December 1904, in which he gave his paradox in the form of the definition in thirteen words of ' the least ordinal which is not definable in a finite number of words '. Russell published it in a slightly variant form, namely, ' the least integer not nameable in fewer than nineteen syllables '.[2]

Berry lived in Oxford and went to Bagley Wood from time to time. Indeed, in the notes mentioned above Russell recalled that at his first visit he presented Russell with a piece of paper carrying on one side ' The statement on the other side of this paper is false ' and on the other side ' The statement on the other side of this paper is false '. (' We then proceeded to polite conversation ', recalled Russell in his notes.) So perhaps we can credit Berry also with the visiting card paradox; for while the above pair of statements are not paradoxical (though Russell presented them as such in his *1967a*, 146 !),[3] it could have occurred to a mind like Berry's that one was generable by changing one of the instances of ' false ' to ' true '. Jourdain may have been told of this result, for he gave both it and the visiting card paradox their first public appearance late in 1906, in *The Granta* (*1906i*, 124). He reprinted it in a later paper (*1913i*, 311–312), which may have started its circulation among logicians; it is printed again on pages 176–178 below.

The ' friend ' whom Russell had reported to Jourdain the previous April (page 44 above) as suggesting the form of the liar paradox in terms of someone who tells lies in a given period of time was Oliver Strachey (letter to Russell of January 1905, in the Russell Archives). But a rather similar version is contained in Berry's letter of 2 November 1906 and might have been communicated personally to Russell earlier.

[1] A paraphrase of this statement appeared in Feinberg *1967a*, 140. Russell made two different versions of his notes on Berry.

[2] Russell *1908a*, 223 (*1956a*, 60; van Heijenoort *1967a*, 153); see also *PM*, vol. 1, 61. He also published the paradox of the smallest indefinable transfinite ordinal at these locations.

[3] Russell corrected his presentation of the paradox in *The Observer*, 12 March 1967, p. 33.

Jourdain also put that result in *The Granta* in December 1905 as his *1905a*, and it is printed again on pages 175–176 below. The correspondence does not record the transmission of this result to Jourdain, but its independent discovery by him seems unlikely. Perhaps it reached him during one of their occasional meetings.

Berry survives in mathematical logic only through his contacts with and letters to Russell; the location even of Russell's replies to him is unknown. Perhaps this brief description will inspire some news from his kith and kin, and thus lead us to more of his remarkable work.

11. *May–July 1905: existence, selections, and definability*

Jourdain replied on 6 May to some of the points raised by Russell in his letter and notes. One of these was a personal matter. Jourdain had reported in his letter of 25 April that 'Cantor announces his intention of coming to England this Summer. He is bringing over a correspondence with Weierstrass to show me; it should be interesting'. Russell had replied on 28 April: 'I should like to see Cantor when he comes. What is his address in Germany?'. But now Jourdain told Russell that Cantor had had to cancel his visit because of illness (see my *1971a*, 121–125).

On mathematical matters, Jourdain admitted the failure of his own method of solving paradoxes but objected to Russell's unclear criterion for the determination of classes by propositional functions, especially when related to the class W of all ordinals:

> *I do not* (*now*) *claim* to have explained at all the contradiction, but confine myself to the problem of finding *which* these odd classes are ... I cannot but think that your criterion of existence is insufficient, because (e.g.) the type of W 'exists' and is yet a self-contradictory class.

Jourdain's criticism (of the existence rather than the "existence" of classes) was pertinent, as Russell admitted in reply on 15 May:

> I admit that my criterion of 'existence' is inadequate. But I think it is *true*, only that some things look like functions and yet are not functions, and these things don't define classes. But *existence* is a question only arising when we already have a class, and serving to rule out the null class. In the contradictory cases, we get no class at all, not the null class. Thus I should say the type of W does not exist, and in fact there is no such entity: it is not merely a null entity.

Russell was stressing not only that paradoxical classes are not classes at all *but also that paradoxical propositional functions are not actually functions*. Hence for him at this time the comprehension principle (that a propositional function determines a class) was true, in contrast to the impression given elsewhere, such as page 32 above. But in calling the paradoxical classes only ' contradictory ' he was in danger of missing the deeper level of contradiction which the paradoxes embody—that, for example, the property of non-self-identity is not satisfied by any object and so determines the empty class, while the property of non-self-membership is satisfied by many classes but leads to the *double* contradiction involved in Russell's paradox and so for Russell does not determine any class at all (a point he made on page 33 above). However, once again he did point out the difference between the empty class and no class.

Jourdain also recalled Russell's attempt the previous June to require ' elementary ' classes to satisfy Cantor's power-class theorem (see page 36 above), and he tried to relate it to paradoxical classes. Russell explained how he saw the theorem using classes near to the bounds of legitimacy :

> A class very like $x\ni(x\sim\varepsilon x)$ does in fact occur in the proof of $2^\alpha>\alpha$. For the nerve of the proof is :
>
> $$R\varepsilon 1\rightarrow 1\ .\ \mathrm{D}`R=u\ .\ \breve{\mathrm{D}}`R\subset \mathrm{Cls}`u\ .$$
>
> $$w=\dot{x}\{x\varepsilon u\ .\ \sim(x\varepsilon\,{}_1`\overleftarrow{R}`x)\}\ .\ \supset\ .\ w\sim\varepsilon\breve{\mathrm{D}}`R.$$
>
> Here $\overleftarrow{R}`x$ means the class of relata of $x$, ${}_1`\overleftarrow{R}`x$ means the only member of this class (since $R\varepsilon 1\rightarrow 1$), and this only member is itself a class, since $\breve{\mathrm{D}}`R\subset\mathrm{Cls}`u$. Here also
>
> $$w=u\cap\dot{x}(x\sim\varepsilon_1`\overleftarrow{R}`x).^1$$
>
> You see that the definition of w is much like $\dot{x}(x\sim\varepsilon)x$; and if w is not a proper class, $2^\alpha>\alpha$ may not be true.

Jourdain's letter contained a defence of his belief in the avoidability of an axiom of choice in the proofs of both Hardy's theorem about the cardinality of the continuum and of Cantor's well-ordering principle.

[1] Instead of ' ε ', Russell accidentally wrote ' R ' in this equation, which yields Λ for w. The equation could be read more easily as :

$$w=u\cap\dot{x}\{x(\sim\varepsilon)(\,{}_1`\overleftarrow{R}`x)\}.$$

w is the class of members of u which do not belong to the subclasses of u corresponding under R, and its non-emptiness guarantees the non-denumerability of $\mathrm{Cls}`w$. Compare Russell *1906a*, 38 (*1973a*, 146), and Crossley *1973a*.

But the defence contained no new mathematical argument, and Russell repeated his own viewpoint in his reply :

> I am keenly interested by what you say about not needing the existence of the multiplicative class for proving (1) that $2^{\aleph_0}$ is greater than all Alephs or equal to one of them, (2) for proving that every class can be well-ordered. I can't persuade myself that, in your published proof [*1904b*, 63–65], suggested by Hardy, of (1), you avoid this assumption. The simpler case is to prove that every infinite class (i.e. every class whose number is not reached from 0 by Induction) contains an $\aleph_0$. Take away successive terms : it by no means follows that you can take away $\aleph_0$ terms merely because you can take away more than any finite number. To prove this, you would have to be able to pitch on one particular way of subtracting terms one by one ; and this can't in general be done, so far as I can see at present. The argument is exactly the same for any later $\aleph$ and a class not given as well-ordered.

In reply on 17 May 1905, Jourdain reported receiving from Hausdorff an offprint of a recent paper *1904a* on cardinal and ordinal exponentiation. The paper showed some similarity with König's first alleged proof (newly reprinted in *Mathematische Annalen* as *1905b*) that the continuum cannot be well-ordered, in that each author expressed his doubts of the validity when $\mu = \omega$ of the theorem

$$\aleph_\mu^{\aleph_\alpha} = \aleph_\mu 2^{\aleph_\alpha} \tag{11.1}$$

given in F. Bernstein's *1901a*, 49–50 (*1905c*, 150–151), König confining himself to the case where $\alpha = 0$.[1] The difficulty lay in the fact that the sub-classes of a class of cardinality $\aleph_\mu^{\aleph_\alpha}$ would not necessarily be sub-classes of segments when $\mu = \omega$, for then μ would not have an immediately preceding ordinal. They both then doubted König's corollary (independent of but similar to Hardy's theorem) that $2^{\aleph_0}$ is greater than any aleph ; in fact, König withdrew his proof because of the doubts over (11.1). In view of Russell's reservations about Hardy's proof, it seems a little surprising that in his reply of 15 June he accepted that König '*does* prove $2^{\aleph_0} \neq \aleph_1$, so that Cantor's expectation was in any case mistaken '.

Russell also asked Jourdain to have Hausdorff send him an offprint of his paper, and then he recalled a feature of his lectures of 1901–1902 :

[1] See Bernstein's replies *1905b* and *1905d* to Hausdorff and König. In Jourdain's notebooks there is a little correspondence with Bernstein about (11.1) and the paradox. Bernstein's result is today normally restricted to the case when μ is finite. For this and related results (including Hausdorff's recursion formula in *1904a*) see, for example, Bachmann *1955a*, 139–143, or Kuratowski and Mostowski *1968a*, 287–291.

You may remember that in my lectures in 1901–2 I had a relation $P^{\times}$ for which we had

$$P \varepsilon w \cap \mathrm{Rel}^2 \,\mathrm{Excl} \,.\, \supset \,.\, P^{\times} \varepsilon \theta',$$

where θ' is the type of the real numbers when a terminating decimal is distinguished from the recurring decimal which has the same value, e.g. ·5 from ·4̇9̇.

He does not seem to have developed this idea further ; at least, there appear to be no manuscripts related to it in the Russell Archives.

On 5 July Jourdain reported corresponding with König, who ' tells me he has solved your contradiction and has asked for your address '. In his reply of 31 July Russell stated : ' I am curious to see König's solution of my contradiction, though I have given up expecting much of solutions'. The surviving correspondence between König and Jourdain does not seem to clarify König's point,[1] and there is none at all in the Russell Archives, but possibly his ' solution ' was connected with his claim in *1905c*, art. 4 that the distinction between a class as one and a class as many ' completely resolves the paradoxes '. If so, then it would hardly have impressed Russell.

Jourdain also mentioned a recent visit from Hardy. They had discussed ' the difficulties of the multiplicative class ', and Jourdain defended his own views on infinite selections as follows :

> I cannot yet see why a mathematician (who is a little God, living in a world of rough insects, mass-less garden-rollers, and so on, created by him) should be limited to *finite* classes when he wants to define by extension. You seem to agree with this (*The Principles*, art. 71). I wish you would tell me what you think of this.

Russell obliged :

> I don't think the fact that, given any α_0 of real numbers, you can construct a real number not in that α_0, proves that the number of real numbers definable in finite terms is greater than α_0. I rather think that ' definable in finite terms ' may be one of those intensions that don't define a class. It seems plain that we are not sufficiently god-like to be able to specify a class without either enumerating its members or giving a property which is common and peculiar to those members. Even a Creator has to be able to make up his mind what he is creating ; and that is here the difficulty.

[1] In Jourdain's notebooks are Jourdain's reply and König's further answer, and a request from Jourdain the previous October for a copy of König's *1905a*.

Try to prove that if you had $\aleph_0$ pairs of boots, you would have an even number of boots. If right boots and left boots are distinguishable it is easy enough ; if not, not.[1]

I do not hold that definition by extension is *logically* limited to finite classes, but it is humanly, because we are not immortal. I don't think the continuum of real numbers is upset by the multiplicative class difficulty. Also I have hopes that much will be discovered to circumscribe the difficulty ; for all we have at present is a mere absence of proofs of propositions which are very likely to be true. But the first step must be to lay bare exactly what the difficulty is and what its bearings are.

Jourdain also reported to Russell that he and Hardy had discussed a recent paper *1905a* by E. W. Hobson on transfinite arithmetic and the paradoxes. Hobson required that classes, and relationships between classes, be specified by a ' norm ', which he regarded as ' a set of specified conditions '. His standpoint was constructivist in the sense that the conditions were to be finite in number, and it corresponded somewhat to König's (independent) stress in *1906a* on finite definability. He reviewed recent literature, and pointed out situations where the requirement of a norm had not been met—among others, Russell's definition of cardinals (since the defined class of classes allegedly had only one member), Hardy's construction of a class of cardinality $\aleph_1$ and his theorem about cardinals, Jourdain's first proof of the well-ordering principle (for not specifying the one–one correspondence between W and a well-ordered series), and Zermelo's use of a ' covering ' (that is, a selection function) in his proof of the well-ordering principle (Hobson *1905a*, 179–187).

Hobson did not realise that the conditions in a norm would not forbid either naive comprehension or the paradoxes of finite definability, and he offered no alternative methods of proving results involved in infinite selections. His three British victims were to express their reciprocal lack of confidence in papers in the 1906–1907 volume of the *Proceedings of the London Mathematical Society* ; Hardy *1906a* on the construction of his class of cardinality $\aleph_1$, Jourdain *1906b* in defence of his proof and of his theory of " inconsistent " classes, and Russell (the least under attack) *1906a* in clarification of the distinction between the problems of the paradoxes and of the axioms of choice. (A. C. Dixon also contributed *1906a*, in which he discussed paradoxes of

[1] This seems to be the earliest recorded example of Russell using this example to illustrate the multiplicative axiom. Published versions appeared in *1906a*, 47–48 (*1973a*, 157–158), *1911a*, 31 (p. 171 below), and most popularly in *1919a*, 126–127. Jourdain put it in his *1918b*, 67–68 (not in *1911d* or *1916a*).

definability.) ' I have not yet written my answer to Hobson ', Russell told Jourdain in his letter of 31 July, ' but I shall write it some time before very long. I rather agree with you that he doesn't really know what are the points he is raising'.

12. *October–December 1905 : more on selections and definability*

There seems to have been a pause in the correspondence for three months, until Jourdain wrote again on 12 October. He reported that he had been found ineligible for the Fellowship examination at Trinity because ' I had degraded [missed a year through illness] when I was up (on account of health) ', and apparently announced his intention to apply for a research studentship at Emmanuel College. He also described some of his recent correspondence with Cantor,[1] recommended a recent paper *1905a* on transfinite arithmetic by A. E. Harward, and enclosed his recent contributions (*1905c* and *1905f*) to the history of mathematical analysis. Russell replied on 2 November :

> Certainly you may use my name as witness to your ' competence for research '. I do hope you will be successful in applying for the Emmanuel Research Studentship . . .
>
> I have written a long paper on Hobson, but it has not yet left my desk. I believe I can now deal satisfactorily with all the various contradictions ; I do so by wholly denying that there are such things as classes and relations, which I treat in the same way as I treated denoting phrases in the current *Mind*, of which I will send you an offprint as soon as I get one.
>
> What you tell me about Cantor is very interesting.
>
> If my present plan of discarding classes is right, the existence-theorems all need new proofs, and some existence-theorems will fail. That is as it should be ; but I hope not very many will fail.

So Russell had just published ' On denoting ', and had been able to use its ideas to develop a method of avoiding the paradoxes. Important news ; but there was no discussion of his new method for some weeks, for in the meantime he had to deal with two more determined efforts by Jourdain at avoiding an axiom of choice.

[1] Especially that Cantor possessed a naive series of transfinite ordinals *before* his discovery that the continuum was not denumerable (see the texts in my *1971a*, 123–124). Cantor published a footnote relating to this point in his *1880a*, 358 (probably in retort to du Bois Reymond *1880a*, 128), but most unfortunately Zermelo omitted it from p. 148 of his edition of Cantor's *Papers*.

Both arguments related to Hardy's work. The first, given in a letter of 13 November, ran as follows :

> I will prove : if M is an infinite aggregate, we can always take away from M an aggregate which can be well-ordered in a series of type ω_1. And this series is *uniquely* determined so soon as m (its first term) is.
>
> Let $M_1 = M \sim \iota m$,[1] and image M on M_1. Then $m \varepsilon M$, $m \sim \varepsilon M_1$ and M_1 is imaged on a proper part ($M_1 \sim \iota m_2 = M_2$ say ; there *are* such m_2) of itself by *the same imaging* ; then $m_2 \varepsilon M_1$, $m_2 \sim \varepsilon M_1$, and so on ; as soon as the original imaging and m are fixed, a ' chain ' $m_1, m_2, m_3, \ldots$ of type ω (at least) is defined uniquely.
>
> Since, then, M has an $\aleph_0$ as part, the Schröder–Bernstein theorem $\supset$:.
>
> $$M \varepsilon \mathrm{Cls\ inf} . \supset . \mathrm{Nc}'M \geqslant \aleph_0.$$[2]
>
> But we can't go on this way. If $\mathrm{Nc}'M > \aleph_0$, a remnant[3] exists after $m_1, m_2, \ldots$ are taken away, but the above imaging (ϕ say) *transforms R into itself* (for if there is an element r of R not in $\phi(R)$, we should have an M *besides* m not in $\phi(M)$).
>
> The above process, as I recognised afterwards, is identical with that of Dedekind [*1888a*, art. 68] for separating out an ω from any infinite aggregate (N.B., his process can't be extended to get an ω_1, e.g.), and with the method[4] at bottom of the Schröder–Bernstein theorem.
>
> So I have thought out another way of proving Hardy's result and my own ; perhaps you will tell me if it is valid. What I feel is that in some questions (like this), though we may not define a ' norm ' to establish the correspondence of N and M, yet we know independently of this that (N sim M) or ($N \sim$ sim M).
>
> Let $\mathrm{Nc}'M > \aleph_0$; to prove that part (or all) of M is similar to [the] field of an ω_1.

[1] This tilde denotes class complementation. Jourdain used the symbol ' , ' in this passage to denote ' the unit class of ', but I have rendered it as usual as ' ι ' to avoid confusion with ' the '.

[2] The Schröder–Bernstein theorem asserts that if a class α is similar to a sub-class of another class β and vice versa, then α and β are similar. It was announced without proof in Cantor *1895a*, art. 2, thm. B. The earliest proofs are by Dedekind *1887a* (unpublished ; see *1888a*, art. 72, and his 1899 proof to Cantor in Cantor *Papers*, 449), Schröder *1898a*, esp. pp. 337–341 (defective ; see Korselt *1911a*), and F. Bernstein (published in Borel *1898a*, 103–106 and given at Cantor's seminar in 1896). Jourdain devoted his *1907a* to this theorem and its relationship to axioms of choice. It was quite prominent in the development of transfinite arithmetic for a number of years. For discussion and references see Fraenkel *1953a*, 97–105, and Medvedev *1966a*.

[3] ' Remnant ' is my conjectural completion of a barely legible abbreviation.

[4] ' Method ' is my chosen rendering of Jourdain's scribble. Presumably he had in mind the use of successive selections in some early proofs of the theorem.

Consider a series of type ω_1. Let us define : 'M can cover a series of type α' to mean : 'a part (or all) of M can be put in a $(1 \to 1)$ with *all* the elements preceding the αth'.

Consider the *logical* alternatives : (1) M can cover ω_1 ; (2) M cannot. (1) implies $\mathrm{Nc}'M \geqslant \aleph_1$ (by Schröder–Bernstein theorem). Consider (2).

If M cannot cover the ω_1, there is a lowest element (β) it can't cover (for $\omega_1 \varepsilon \Omega$) ; then in *no* imaging can it cover a series of type $\geqslant \beta$. ***But there can be no such*** β in the ω_1, for the series of elements $\leqslant \beta$ is an $\aleph_0$ and can therefore be covered, since M has an $\aleph_0$ as part. Hence M *can* cover the ω_1, and $\mathrm{Nc}'M \geqslant \aleph_1$.

The proof in general

$$(\aleph_\gamma < \aleph_\alpha \,.\, \supset \,.\, \mathrm{Nc}'M > \aleph_\gamma : \supset_\alpha \,.\, \mathrm{Nc}'M \geqslant \aleph_\alpha)$$

is similar (ω_α used for ω_1).

Jourdain's argument falls into two parts ; that M contains a denumerable sub-class, and that it contains an order-type ω_1. To the modern view it is clear that at least the second part assumes an axiom of choice. Perhaps with its use of the reflexive sense of infinity in mind (compare Cantor's proof in *1895a*, art. 6) Russell accepted the first part in his reply of 17 November, but he detected the assumption in the second part :

> Your argument that an infinite aggregate must always contain an $\aleph_0$ is perfectly valid, at least I believe it to be so. But your further argument appears to me to involve a covert use of the multiplicative axiom. You argue 'If M can't cover an ω_1, there must be a smaller ordinal which is the first it can't cover'. I presume you hold this because, otherwise, the successive coverings would give an ω_1. But this requires us to pick out one each of the various ways in which the M can cover an α, where α is a given ordinal. Put
>
> $$\mathrm{less}'\alpha = \text{ordinals less than } \alpha \quad \text{Df.}$$
>
> Then 'M can cover a series of type α'
>
> $$= \,.\, (\exists R) \,.\, R \varepsilon 1 \to 1 \,.\, \mathrm{D}'R \subset M \,.\, \breve{\mathrm{D}}'R = \mathrm{less}'\alpha.$$
>
> Put
>
> $$\mathrm{Cover}_M'\alpha = \dot{R}\{R \varepsilon 1 \to 1 \,.\, \mathrm{D}'R \subset M \,.\, \breve{\mathrm{D}}'R = \mathrm{less}'\alpha\} \quad [\text{Df.}]$$
>
> Then you need a member of
>
> $$X'\mathrm{Cover}_M{}''\mathrm{less}'\omega_1$$

in order to infer $\exists \mathrm{Cover}_M{}'\omega_1$ from

$$\alpha \varepsilon \mathrm{less}'\omega_1 . \supset_\alpha . \mathrm{Cover}_M{}'\alpha.$$

I don't see otherwise how your conclusion from all the predecessors of ω_1 to ω_1 is to be got.

And would not your argument, apart from this, prove that an $\aleph_0$ can cover an ω_1? For it can cover all the predecessors of ω_1, since these are all $\aleph_0$'s. I dare say I have missed your point; if so, would you mind writing out the argument rather more fully?

Russell went on to comment on some remarks (now mostly lost) by Jourdain on the denumerability of the class of definable objects:

> As regards your argument about the aggregate of definable entities, there appears to me to be a confusion between defining a class and defining all its members. E.g. the class composed of all classes of finite integers has a finite definition, but some of its members (most of them, in fact) can't be finitely defined. Conversely, each of a set of ordinals might be finitely definable and yet the class of them might not be.[1] I don't know if this case ever arises, but I don't see why it shouldn't. I hear, however, that a forthcoming paper of Hobson's upsets this whole argument and overcomes the difficulty.

Russell must have been as disappointed with the new paper by Hobson as he was with its predecessor, for Hobson spoke of definability without specifying the nature of the language involved (for example, whether or not it contained variables) and included the temporal consideration that a 'lawless decimal' cannot be considered as a whole, but only that part of it which has been specified by some particular time.[2]

Russell also remarked in his letter: 'Your puzzle about the weights is pleasant and amusing'. Jourdain may have sent to Russell his illustration (published in his *1910b*) of the Dedekind cut by the statement that the greatest weight that a man can lift is the least weight that he cannot lift.

Jourdain's second attempt at avoiding infinite selections was sent to Russell on 9 December 1905:

[1] Russell seems to write both 'class' and 'set' here only for stylistic reasons.

[2] Hobson *1906a*, esp. pp. 26–28; see also Dixon *1906b*. Hobson's talk of lawless decimals, already in his *1905a*, is an influence of du Bois Reymond *1882a*. Russell dismissed Hobson's *1906a* summarily in his *1908a*, 223 (*1956a*, 60; van Heijenoort *1967a*, 153), but was more generous in *PM*, vol. 1, 61 (as Jourdain noticed; see p. 183 below).

Will you tell me if this account of where we assume existence of a multiplicative class in, e.g., Hardy's proof that

$$\mathrm{Nc}`u > \aleph_0 \,.\, \supset \,.\, \mathrm{Nc}`u \geqslant \aleph_1 \qquad [(12.1)]$$

is correct ?

If we are given $\mathrm{Nc}`u > \aleph_0$, then it seems to follow that, if α is any ordinal of [the] second class, there is a part u_α of u such that

$$u_\alpha \text{ sim } \{\dot{\gamma}(\gamma < \alpha)\},$$

and also these u_α's are such that if β is of [the] second class and $\beta > \alpha$, there is a part u_β of u such that u_α is a *proper* part of u_β. The choice of the u_α is, to a certain extent, arbitrary. Let V_α be the class of such u_α's (for a definite α) ; then if $V =$ class of all such V_α's (for all the α's) we need an element (v (a class of different elements of u corresponding to all the numbers $< \omega_1$)) of $V^\times$, so we need to prove $\exists V^\times$. (Is V a Cls[2] excl ? It does not seem to be so.)

Our object is to get v : is it *only* definable as an element of $V^\times$?[1] Would you mind making this clear to me, as I can't yet see why, if it is *possible* to choose a u_α, it is not possible to choose (theoretically) *one* definite u_α. If, now, it is possible to choose a definite u_α, where α is any number of [the] second class, such that

$$\beta > \alpha \,.\, \supset \,.\, u_\beta \underset{\text{prop}}{\subset} u_\alpha$$

$$(a, b \varepsilon \mathrm{Cls} \,.\, \supset : a \underset{\text{prop}}{\subset} b \,.\, = \,.\, a \subset b \,.\, \exists b \cap \dot{x}(x \sim \varepsilon a) \quad \mathrm{Df})^2$$

we can define a part of u (v) which is an $\aleph_1$ as follows :

$$v = \dot{x}(\exists \mathrm{No} \text{ of second class} \cap \dot{\alpha}(x \varepsilon u_\alpha)) \quad \mathrm{Df}.$$

Evidently $\exists v$, as we can fix upon *one* member of v (any member of u we like to fix on).

Is there any objection to the arbitrary selection of a *finite* number of members from u ?

The main steps of Jourdain's argument are as follows. For an ordinal α of the second number-class there exists a sub-class u_α of u such that

$$u_\alpha \text{ sim } \{\dot{\gamma}(\gamma < \alpha)\} \qquad (12.2)$$

[1] Jourdain put a footnote here : ' I.e. I don't see why we should not be able to form v, and yet $V^\times$ is not a class ; just as there are things x which $\sim \varepsilon x$ and yet $\dot{x}(x \sim \varepsilon x)$ is not a class '.

[2] In this correspondence ' $\subset$ ' denotes *im*proper inclusion, a relation symbolised today by ' $\subseteq$ '.

and an ordinal β of the second number-class such that

$$\beta > \alpha \,.\, \supset : u_\alpha \underset{\text{prop}}{\subset} u_\beta \,.\, u_\beta \subset u. \tag{12.3}$$

Since u_α is not necessarily unique for a given α, we define the classes

$$V_\alpha = \dot{s}\{s \subset u \,.\, s \text{ sim } (\dot{\gamma}(\gamma < \alpha))\} \quad \text{Df} \tag{12.4}$$

and

$$V = \dot{w}\{(\exists \alpha) \,.\, \alpha \varepsilon \text{No}(\aleph_0) \,.\, w = V_\alpha\} \quad \text{Df.} \tag{12.5}$$

We now seek a choice class v for V which therefore belongs to $V^\times$ (the multiplicative class of V) and also is a sub-class u_{ω_1} of u (since V is defined from all ordinals of the second number-class), thus establishing the lemma (12.1) to Hardy's theorem. However, the construction of $V^\times$ might involve an axiom of choice, and V is not necessarily a class of mutually exclusive classes anyway. So we choose a u_α satisfying (12.3) and define v without resorting to $V^\times$ as follows :

$$v = \dot{x}\{\text{No}(\aleph_0) \cap \dot{\alpha}(x \varepsilon u_\alpha) \neq \Lambda\} \quad \text{Df.} \tag{12.6}$$

(12.3) guarantees that if $x \varepsilon u_\alpha$ then $x \varepsilon u_\beta$ also, and so on right through the second number-class. v is the class of members of u which belong to at least one u_α, and so (for Jourdain) (12.1) is established without recourse to an axiom of choice.

This proof is noteworthy as an early example of the construction of a maximal class. Such methods were not to become well-known until Zermelo's second proof (*1908a*, art. 1) of the well-ordering principle by the use of a ' θ-chain ' (the class of nested sub-classes of a class and their intersections, a generalisation of Dedekind's chain in *1888a*, art. 37), and especially with Hausdorff's efforts to use maximal principles to avoid assuming both an axiom of choice and transfinite induction.[1] Relative to these later developments (which were later discovered to be equivalent to assuming axioms of choice) Jourdain's approach was rudimentary. Russell did not have too much difficulty on 15 December in finding loop-holes :

> I think the multiplicative axiom does, but for what I think is a previous fallacy, come in at the point you indicate, but it has already come in earlier [sic]. Put $\text{No}(\aleph_0)$ = ordinal of the second class.

[1] See Hausdorff *1914a*, esp. pp. 133–142 ; compare *1962a*, 65–67, and the editorial remarks on pp. 338–339. For references to relevant literature and discussion of principal theorems see, for example, Rubin and Rubin *1963a*, 10–37, and Kuratowski and Mostowski *1968a*, 261–269.

Then your u_α, where u_α sim $\dot\gamma(\gamma<\alpha)$, is, I take it, not quite accurately expressed ; for

$$\alpha\varepsilon\mathrm{No}(\aleph_0)\ .\ \supset\ .\ \dot\gamma(\gamma<\alpha)\varepsilon\aleph_0\ ;^{1}$$

hence

$$u_\alpha \text{ sim } \dot\gamma(\gamma<\alpha)\ .\ \equiv\ .\ u_\alpha\varepsilon\aleph_0.$$

What you mean to get at is a *series* of type α, continued beyond until it becomes of type β, and so on. Now of course

$$u\varepsilon\mathrm{Cls\ infin}\ .\ \supset\ :\alpha\varepsilon\mathrm{No}(\aleph_0)\ .\ \supset_\alpha\ .\ (\exists R)\ .\ R\varepsilon\alpha\ .\ \mathrm{C}`R\subset u.$$

But if you put

$$u_\alpha=\dot v\{(\exists R)\ .\ R\varepsilon\alpha\ .\ \mathrm{C}`R\subset u\ .\ \mathrm{C}`R=v\}\quad[\mathrm{Df}]$$

you have

$$\alpha\varepsilon\mathrm{No}(\aleph_0)\ .\ \supset_\alpha\ .\ u_\alpha=(\mathrm{Cls}`u)\cap\aleph_0.$$

The process you want is the process of picking out terms one by one. This is practically Zermelo's process, and requires Zermelo's axiom . . .

Consider the argument in words : One says ' Terms can be picked out one by one from u, and either this process comes to an end with some $\mathrm{No}(\aleph_0)$, or it goes on at least until we have an ω_1 '. I will suppose an ω already picked out : then we may choose our next term in an infinite number of ways, our next after that in an infinite number of ways, and so on ; to get the result we want, we must at each stage pick out *one* of the infinite number of ways open to us. Certainly your class will contain a series of type α, where α is any $\mathrm{No}(\aleph_0)$; for it contains an $\aleph_0$, and any $\aleph_0$ can be arranged so as to have the type α. But no $\mathrm{No}(\aleph_0)$ is appreciably nearer to ω_1 than its predecessors, to speak loosely.

(In your case, where you put

$$V_\alpha=\dot v\{v\subset u\ .\ v \text{ sim } \dot\gamma(\gamma<\alpha)\}\quad[\mathrm{Df}],$$

$$V=\dot w\{(\exists\alpha)\ .\ \alpha\varepsilon\mathrm{No}(\aleph_0)\ .\ w=V_\alpha\}\quad[\mathrm{Df}].$$

one has

$$\alpha\varepsilon\mathrm{No}(\aleph_0)\ .\ \supset\ .\ V=\iota`V_\alpha,$$

which was not intended.[2])

The process of picking out one term and then another and so on, and thus getting well-ordered series contained in a given class,

[1] Russell used ' $\aleph_0$ ' here to refer to the class of denumerable classes. $\dot\gamma(\gamma<\alpha)$ is denumerable by virtue of the definition of membership of the second number-class.

[2] For

$$V_\alpha=\dot v\{v\subset u\ .\ v\varepsilon\aleph_0\},$$

which is the class P of denumerable sub-classes of u for *any* α. But then $V=\iota`P$.

is essentially the process of Zermelo's proof that every class can be well-ordered, which involves the axiom

$$u \varepsilon \mathrm{Cls} \,.\, \supset : (\exists f) : w \subset u \,.\, \exists w \,.\, \supset_w . f'w \varepsilon w.$$ [1]

To get the multiplicative axiom from this, take

$$k \varepsilon \mathrm{Cls}^2 \,\mathrm{excl} \,.\, \mathrm{U}'k = u,$$ [2]

then $f''k \varepsilon X'k$ whence $\exists X'k$.

$$(f''k = \dot{x}\{(\exists w) \,.\, w \varepsilon k \,.\, x = f'w\} \quad \text{Df.})$$

For by the definition of u, if w is a member of k then it is a sub-class of u. Now the selection function f is so specified that as w ranges through the members of k, x correspondingly generates a choice class. But this class is $f''k$, which therefore belongs to the multiplicative class $X'k$ of k. Hence $X'k$ is non-empty. Russell treated the theory of choice classes in broadly the same way in *Principia Mathematica*, ✳83, though primacy was naturally given to the selection functions under which the classes were subsumed. This passage shows that already he had full mastery of the technicalities.

Russell appended to this analysis the remark : ' But I see no reason at present for admitting Zermelo's axiom, which is even more of an assumption than the multiplicative axiom '. He saw his multiplicative axiom as a narrower assumption than Zermelo's axiom of choice (presumably because of its restrictions of k to classes of mutually exclusive classes, and of the intersection of each member of k with a choice class to a unit class) ; and in his reply to Hobson he incorporated a proof of this statement.[3] He continued this letter with some examples of finding a choice class (that is, a member of a multiplicative class) without making use of an axiom of choice :

> In particular cases, where k has some given structure, one can often construct a term of $X'k$, whence $\exists X'k$ follows. In such cases, no axiom is needed. E.g. we have
>
> $$k \varepsilon \mathrm{Cls}^2 \,\mathrm{excl} \,.\, \supset \,.\, k \varepsilon X'\mathrm{Cls}''k$$
>
> whence
>
> $$k \varepsilon \mathrm{Cls}^2 \,\mathrm{excl} \,.\, \supset \,.\, \exists X'\mathrm{Cls}''k.$$
>
> $$(\mathrm{Cls}''k = \dot{w}\{(\exists u) \,.\, u \varepsilon k \,.\, w = \mathrm{Cls}'u\} \quad \text{Df.})$$

[1] Note that ' $\exists w$ ' expresses the " existence " of w. ' w ' is only universally quantified in this formula.

[2] ' $\mathrm{U}'k$ ' is Russell's current notation for the ' sum ' of k, that is, the class of members of some member of k. Compare *PM*, ✳40.02, with its new notation :

$$s'k = \hat{x}\{(\exists \alpha) \,.\, x \varepsilon \alpha \,.\, \alpha \varepsilon k\} \quad \text{Df.}$$

[3] Russell *1908a*, 51–52 (*1973a*, 161–162) ; compare *PM*, ✳88.03.

If you can find such a construction in the cases that interest you, you will have proved the theorems concerned ; but in the absence of any particular known construction, the proofs fail unless Zermelo's or the multiplicative axiom is assumed. The difficulty is quite unlike that of *inconsistent* aggregates ; if you can find a member of the multiplicative class, well and good ; if there is no member, the multiplicative class is Λ, not inconsistent. E.g. consider $\aleph_0$ pairs of boots, and ask yourself how many ways there are of dividing the boots into a pair of $\aleph_0$'s. If the multiplicative axiom fails in that case, the answer is 0, i.e. there are no ways.

There is no objection to selecting a finite number of members from u, because in this case existence can be proved by induction.

These examples of special cases which avoid an axiom of choice relate to an earlier example in the letter, which we omitted in order to reserve it for its special place here. In his previous letter on page 58 above Russell had accepted from Jourdain a proof, avoiding the use of such an axiom, that an infinite class contains a denumerable sub-class. Now he gave a more refined proof using the reflexive definition of infinity (in contrast to the need of an axiom of choice on page 53 above with the non-inductive definition of infinity) and Frege's ancestral relations.[1] As part of his study of the logic of relations Russell had independently developed techniques similar to Frege's in his treatment of mathematical induction,[2] and now he applied Frege's theory as follows :

There is a proof, without this axiom, that every Cls infin contains an $\aleph_0$, for we have

$$\text{Cls infin} = \dot{u}\{(\exists R) \,.\, R \varepsilon 1 \to 1 \,.\, \text{D}`R = u \,.\, \breve{\text{D}}`R \subset u \,.\, \exists u - \breve{\text{D}}`R\} \quad \text{Df.}^3$$

Put

$$\overleftarrow{R^{**}}`x = \dot{q}\{x \varepsilon s : y \varepsilon s \,.\, yRz \,.\, \supset_{y,z} \,.\, z \varepsilon s : \supset_{s} \,.\, q \varepsilon s\} \quad \text{Df.} \quad [(12.7)]$$

Then

$$R \varepsilon 1 \to 1 \,.\, \breve{\text{D}}`R \subset \text{D}`R \,.\, x \varepsilon \text{D}`R - \breve{\text{D}}`R \,.\, \supset \,.\, \overleftarrow{R^{**}}`x \varepsilon \aleph_0$$

whence

$$u \varepsilon \text{Cls infin} \,.\, \supset \,.\, \exists \aleph_0 \cap \text{Cls}`u.$$

[1] For Frege's theory see *1879a*, arts. 24–29, *1884a*, arts. 82–83, and *1893a*, arts. 45–46.

[2] See especially Russell *1901c*, arts. 3–4, and *1902a*, 32–33. Russell may have been influenced by Dedekind's chain (a somewhat similar theory published later in *1888a* but developed independently), for there are Russell's notes on Dedekind's *1888a* in the Russell Archives which seem early. But in *1903a*, art. 495 he acknowledged Frege's superiority.

[3] This is the reflexive definition of infinity. Since ' $\subset$ ' denotes improper inclusion, it is necessary to state the " existence " (' $\exists$ ') of $(u - \breve{\text{D}}`R)$ to obtain proper inclusion.

But if you try to get this result by picking out terms one by one, you have to use Zermelo's axiom.

For Russell the ancestral relation of R relates members of a class s which is ' hereditary ' with respect to R, that is, which contains the class of relata of its members under R (as stated in the central clause of his definition (12.7) of $\breve{R}^{*}`x$). In (12.7) he followed Frege in not requiring $x$ to belong to the field of $R$, but by the time of *Principia Mathematica* he modified the definition of the ancestral relation to apply only to members of the field of $R$, so that the definition of $\breve{R}^{*}`x$ corresponding to (12.7) reads :

$$\breve{R}^{*}`x = \grave{q}\{x \varepsilon \mathrm{C}`R : y \varepsilon s \,.\, yRz \,.\, \supset_{y,z} \,.\, z \varepsilon s : x \varepsilon s \,.\, \supset_{s} \,.\, q \varepsilon s\} \quad \text{Df.}^{1} \qquad (12.8)$$

One purpose of the ancestral relation is to provide a means of expressing mathematical induction by analysing the powers R^{n} of R as applied to hereditary classes. $\breve{R}^{*}`x$ is such a class (✳90.163), and (assuming that $R$ is not many-one and not reflexive) its denumerability follows from starting with a member of $(\mathrm{D}`R - \breve{\mathrm{D}}`R)$ and generating a denumerable series of different members by taking successive powers of $R$. However, as Russell was careful to point out, this result did not fulfil Jourdain's hope of avoiding axioms of choice in mathematics. Russell dealt extensively with ancestral relations and $\breve{R}^{*}`x$ in *Principia Mathematica* (✳90–✳96 *passim*), but he stressed that results such as the existence of a denumerable sub-class in an infinite (in the non-inductive sense) class could not be proved *in general* without the multiplicative axiom (see ✳261.401, and vol. 3, 120).

Russell concluded his letter of 15 December 1905 with an answer to an enquiry by Jourdain about König's second proof that the continuum was not well-ordered. We recall from page 49 above that König's proof hinged on consideration of the first element in the complement with respect to the continuum of the class of finitely defined real numbers. Jourdain had finished his letter :

> Finally, does König's conclusion (*1905c*, 158–159 [van Heijenoort *1967a*, 147–148]) . . . strike you as valid ? Can the phrase ' first number after a well-ordered series of numbers to which cardinal

[1] *PM*, ✳90.11 and ✳32.131, together with ✳37.17. Russell wrote ' $\breve{R}_{*}`x$ ' there for this class. He specified $R$ in (12.7) as one-one, a condition which applies presumably in (12.8) also. The point is of some importance, for if $R$ is (many-one and) reflexive, $\breve{R}_{*}`x = \iota`x$ (*PM*, ✳96.33). He might have used Frege's proper ancestral (introduced in *1879a*, art. 26), written by Russell as ' R_{po} ' and defined as the product of R_{*} and R (*PM*, ✳91.52 with ✳91.05 ; see also ✳91.542). On the two kinds of ancestral relation, see Quine *1969a*, 101–107.

$\aleph_0$ belongs ' be expressed with a finite number of repetitions of the fundamental notions ($\supset$, ε, rel . .) ?

Russell concluded his answering letter :

> I have not yet seen König's article. I hear Hobson has an article refuting it (not yet published). It does not strike me as valid ; because ' finitely defined ' cannot, without a vicious circle, be included in our indefinables, or defined in terms of them.

The paper by Hobson is the one already mentioned by Russell in his letter of 17 November on page 59 above, where we noted that Russell later recorded his disappointment. His own remarks about Jourdain's account of König's argument are interesting because they show him thinking about vicious circles in connection with the paradoxes. He had been considering vicious circles since 1904,[1] and claimed in the above passage that finite definability can be neither an indefinable of the system nor be finitely defined in terms of its indefinables without incurring a vicious circle. But it is not obvious why vicious circularity is involved in either case, or why, to quote from the paper of 1906 where he publicly accepted the vicious circle principle, ' the notion of *definition* . . . is not definable, and is indeed not a definite notion at all ' (*1906c*, 645 (*1973a*, 209)). We shall return to this point on page 90 below when we deal with Russell's letters of that time to Jourdain.

13. *December 1905–January 1906 : denoting and the mysteries of existence*

Jourdain answered Russell's last letter on 28 December, explaining another aspect of his theory of " inconsistent " classes :

> From a letter of Hardy's I gather that you point out, in your reply to Hobson, that I cannot consistently define cardinals as classes, since they are inconsistent classes which you say are not classes at all.[2] But I think an ' inconsistent ' (it is a bad name but any word will do) class *is* a class : it is Nc'u ($u \varepsilon$Cls inconsist) that is not . . .
>
> I write
>
> $$\sim \mathrm{E}u = u \varepsilon \Lambda \quad \mathrm{Df} \qquad [(13.1)]$$

[1] See especially the manuscript ' On functions, classes and relations ' (1904), and the worksheets ' FN ', esp. f. 564.

[2] See Russell *1906a*, 43 (*1973a*, 152). At the time of this letter, Russell's *1906a* had not yet been published.

and expressed myself at first 'u does not exist' and so got mixed up with your $\exists$. But

$$u\varepsilon\mathrm{Cls} \,.\, \supset \,.\, \mathrm{E}u$$

so $\mathrm{E}\Lambda$; but $\sim\exists\Lambda$.

On the other hand, apparently we have

$$\mathrm{No}\varepsilon\mathrm{Nc}`\mathrm{No},$$

so $\exists\mathrm{Nc}`\mathrm{No}$. But $\sim\mathrm{ENc}`\mathrm{No}$. So I urged that your criterion of existence was insufficient for mathematical purposes, for in mathematics we need to know (e.g.): *Is* Nc'No a class? If so, then $\exists\mathrm{Nc}`\mathrm{No}$; but Nc'No is contradictory—a paradox solved by denying that it is a class (any more than your $\hat{x}(x\sim\varepsilon x)$), or, what comes to the same thing, by proving that $\sim\mathrm{ENc}`\mathrm{No}$.

Now my definition of an 'inconsistent' class *is*

$$\mathrm{Cls\ inconsist} = \mathrm{Cls}\cap x\ni(\sim\mathrm{ENc}`x), \quad \mathrm{Df}$$

$$(x\varepsilon\mathrm{Cls\ inconsist} \,.\, \equiv : x\varepsilon\mathrm{Cls} \,.\, \sim\mathrm{ENc}`x)$$

and *not*

$$\mathrm{Cls\ inconsist} = \mathrm{Cls}\cap x\ni(\sim\mathrm{E}x) \quad \mathrm{Df}$$

an absurd definition with which Hobson (e.g.) credits me [*1905a*, 171] (absurd because if we say (as I do) $\mathrm{No}\varepsilon\mathrm{Cls\ inconsist}$, the hypothesis $\gamma\varepsilon\mathrm{No}$, which is necessary to symbolism [sic], becomes meaningless).

This class is, I think, a good class, but it is inconsistent for Nc'Cls is not a class.

Russell replied on 1 January 1906. He pointed out the inadequacy of Jourdain's definition (13.1) of the non-existence of classes and rejected " inconsistent " classes as classes of any sort:

> In my reply to Hobson, I did not directly (or intentionally indirectly) accuse you of any inconsistency. But in speaking of the theory that inconsistent classes are such as cannot be ordered according to a segment of the series of ordinals, I pointed out that in that case Nc'u is an inconsistent class (provided $\exists u$), because Nc'u is a class which has as many terms as there are entities altogether.
>
> I cannot follow what you say about Eu, because $\sim\mathrm{E}u$ is self-contradictory, since $(x) \,.\, x\sim\varepsilon\Lambda$.[1] Will you explain this further?

[1] And then $u\sim\varepsilon\Lambda$, whereas $u\varepsilon\Lambda$ in (13.1). Compare *PM*, vol. 1, 216: '"$x\varepsilon\Lambda$" will be meaningless, not false'. For some reason Jourdain revived his definition in his *1917b*, 149–150.

My criterion of existence was formerly insufficient, but not my criterion of $\exists$. What was wrong was assuming individuals which have no being. I think it is demonstrable that inconsistent classes are not classes or anything else, but are merely mistakes. I now extend this to *all* classes. The error seems to me to lie in supposing that many entities ever combine to form one new entity, the class composed of them. In the particular case of inconsistent classes, this can (I think) be *proved* ; in other cases I adopt it as the simplest and most convenient hypothesis. Thus many x's satisfy $x \sim \varepsilon x$, but there is no such entity as $\hat{x}(x \sim \varepsilon x)$.

Russell made these points partly in connection with the following suggestion in Jourdain's letter that the multiplicative class be not interpreted as a class. Jourdain had written :

Now it occurred to me that

$$k \varepsilon \mathrm{Cls}^2 \text{ excl } . \sim \supset . \mathrm{E}(\breve{\mathrm{C}}'k) ;$$

or that the [multiplicative class] may not be a class. In this case, to say $\exists(\breve{\mathrm{C}}'k)$ or $\sim\exists(\breve{\mathrm{C}}'k)$ is meaningless ; but it is still a question whether there is any one class (m) of one element out of each y, where $y \varepsilon k$.[1] If so and $\breve{\mathrm{C}}'k \varepsilon \mathrm{Cls}$, then $\exists(\breve{\mathrm{C}}'k)$.

Ought we not to state the axiom in a form not implying that $\breve{\mathrm{C}}'k \varepsilon \mathrm{Cls}$? This is what I would suggest. We have this state of things :

Let u be a Cls infin, to *each* x (where $x \varepsilon u$) correlated in the manner of a many-valued function, where u is the argument-aggregate, a class (in general infinite) denoted $f(x)$. We require to find a one-valued function (a $\mathrm{Nc} \to 1$ relation) which correlates *one* member of $f(x)$ to each x.

This is done if we assume, as [a] primitive proposition, that from every $\mathrm{Nc} \to \mathrm{Nc}$ relation (R say) can be decomposed into a class of $\mathrm{Nc} \to 1$ relations such as S, when $\mathrm{D}'S = \mathrm{D}'R$; $\breve{\mathrm{D}}'S$ is one out of each of [the] classes represented above by $f(x)$.[2]

Russell concluded his letter of 1 January with an acceptance of this proposal :

Yes, the multiplicative axiom should be stated so as not to imply that $\breve{\mathrm{C}}'k$ is a class. We want (not introducing my denial of all classes)

[1] Jourdain added the footnote : 'Analogy : there is an x for which $x \sim \varepsilon x$, but $\sim \mathrm{E}(\hat{x}(x \sim \varepsilon x))$; there is no class of all such x's '.

[2] This is the most intelligible rendering of a sentence which Jourdain only partially revised in his draft. He referred to the point made here in *1906f*, 68–69.

$k \varepsilon \mathrm{Cls}^2 \text{ excl} \,.\, \Lambda \sim \varepsilon k \,.\, \supset : (\exists u) : u \subset \mathrm{U}`k : p \varepsilon k \,.\, \supset_p \,.\, p \cap u \varepsilon 1. \quad [(13.2)]$

The form you give, asserting that a Nc→Nc can have a Nc→1 chosen out of it, is one of several alternative forms. We practically never need $X`k \varepsilon \mathrm{Cls}$; what we need is [(13.2)]. I know no way of proving it, and it strikes me as too complicated for a primitive proposition. When I have got on a bit with the development of my ' no-classes ' theory I may be able to discover more about the multiplicative axiom ; but at present I feel quite agnostic about it.

Russell's temporary abandonment of the denial of classes in order to allow their existential quantification in (13.2) is reasonable, but his remarks about $X`k$ not being a class need clarification. The object satisfying the properties in (13.2) is a class, but there is no assumption that $X`k$, the (multiplicative) class of such choice classes, need be regarded as a class. Aware from his paradox of the dangers of class abstraction, he was perhaps concerned by the difficulty of specifying a propositional function which could determine a multiplicative class. He may have had in mind the use of contextual definition to avoid postulating a class. He might even have considered the abstraction of classes without the assumption of their existence, although he did not pursue this approach in detail in his logical studies.[1]

In reply on 10(?) January, Jourdain continued to defend his definition (13.1) of the non-existence of classes, despite Russell's devastating criticism of it :

> Now, of course, even to say ' *u* is a non-entity ' is contradictory, for there *is* no *u* to talk about in that case ; but yet certain phantoms like ' type of *W* ' *do* come in mathematics, and to prove that they *are* phantoms (sometimes a long process), we must talk of ' them ' as if they were not (in fact, we must be a sort of society for psychical research). Thus $\sim \mathrm{E}u$ *is* self-contradictory, but so is any statement about ' it ', such as your ' *u* is a mistake '. Thus I say : ' There seem to be certain things *u*, but of which we can prove $u \varepsilon \Lambda$, we call such things *nullities* ' ; in symbols :
>
> $$\sim \mathrm{E}u \,.\, = \,.\, u \varepsilon \Lambda \quad \mathrm{Df.}$$
>
> (I think that
>
> $$\mathrm{E}u \,.\, \equiv \,.\, u \varepsilon \mathrm{Cls.})$$

[1] It is prominent in Quine's theory of virtual classes (see, for example, *1969a*, 15–21), which takes *PM*, ✳20.3 as its starting point. For manuscripts of Russell on such questions, see the undated ' General theory of classes ' (the manuscript of two with this title which is numbered 230.030770), and the worksheets ' FN ', ff. 680–800 *passim*.

Russell had thought more carefully about the problems raised here by Jourdain, and in his reply of 13 January he brought into the discussion his theory of definite descriptions, recently published in 'On denoting':

> The theory of non-entities is important, and I believe my new theory of denoting solves all the puzzles completely. This theory is roughly as follows: Given a proposition containing a denoting phrase, 'the so-and-so', it is a mistake to suppose that this phrase stands for an entity, which can be regarded as the subject of the proposition. Let $(\imath x)(\phi x)$ stand for 'the x which has the property ϕx'; then if ψx is any property, $\psi\{(\imath x)(\phi x)\}$ is not one of the values of ψx, but stands for
>
> $$(\exists b) : \phi x \,.\, \equiv_x \,.\, x = b : \psi b.^1$$
>
> Thus $(\imath x)(\phi x)$ by itself has no meaning; but any proposition in which it occurs has meaning. Thus $(\imath x)(\phi x)$ is first introduced by
>
> $$\psi\{(\imath x)(\phi x)\} \,.\, = : (\exists b) : \phi x \,.\, \equiv_x \,.\, x = b : \psi b \quad \text{Df.}^2 \quad [(13.3)]$$
>
> We put
>
> $$\mathrm{E}!(\imath x)(\phi x) \,.\, = : (\exists b) : \phi x \,.\, \equiv_x \,.\, x = b \quad \text{Df.} \qquad [(13.4)]$$
>
> If this condition is not fulfilled, we say that $(\imath x)(\phi x)$ is a non-entity; but there is not something which is a non-entity, there is merely a phrase which fails to describe anything.
>
> Note that $\mathrm{E}!x$ is meaningless.
>
> Note also that
>
> $$\mathrm{E}!(\imath x)(\phi x) \,.\, \equiv \,.\, (\imath x)(\phi x) = (\imath x)(\phi x).$$
>
> If u is a class, we write $\imath' u$ for $(\imath x)(x \varepsilon u)$, that is,
>
> $$\psi(\imath' u) \,.\, = : (\exists b) : x \varepsilon u \,.\, \equiv_x \,.\, x = b : \psi b \quad \text{Df.}$$
>
> The above is the sum and substance of the enclosed article on Denoting, p. 491 [*1956a*, 54; *1973a*, 117].

Russell's theory of definite descriptions has been a source of controversy and discussion ever since its publication. Some of the interest has been in its historical background, especially in Russell's debate with A. Meinong on phrases such as 'the golden mountain' which seem to have no referent,[3] and in the relationship between his and

[1] Some definition of identity is assumed here.

[2] Russell wrote this definition in this form in *1908a*, art. 8, but in *PM*, ✳14.01 he prefaced the defined term by '$[(\imath x)(\phi x)]$'.

[3] Jourdain had held a view like Meinong's. In answer on 16 January to this letter from Russell, he reported that he once tried to develop a theory of this kind, in which the empty class was 'the class of all "unreal" things (like a round square)'.

Frege's theories.[1] But most of the literature deals with the philosophical consequences of his theory; for example, his denial that definite descriptions can be singular terms, his distinctions between descriptions and names and between the primary and secondary occurrence of descriptions in sentences, the status of the denoting complex, and so on.[2] The passage above indicates which features of his theory Russell himself saw as the most significant *at the time*; namely, the use of contextual definition in (13.3) to avoid treating descriptions as terms and so assigning to them isolable referents,[3] and the difference between the (Meinongian) view that descriptive phrases always refer to some kind of entity and his own view that some descriptive phrases do not refer to any kind of entity. Noteworthy also is the definition of the existence of a referent for a descriptive phrase.

This new sense of existence needs to be related to the various other kinds of existence which we have seen already. Russell expressed it in his writings in terms such as 'the x which satisfies $\phi\hat{x}$ exists',[4] which may seem at first glance to speak of the existence of an individual in some ontological sense. But Russell stressed very clearly to Jourdain that 'E!x is meaningless',[5] showing that this sense of the word 'existence' is a conventional one, like the non-emptiness sense of the "existence" of classes. Thus we shall similarly endow it with double quotation marks in our commentaries.

We now have five senses of existence appearing in the correspondence; two conventional and three ontological, two concerned with individuals and three with classes. It will be worth listing these five senses, pointing out their chief characteristics and relationships with other senses.

1. Existence of an individual x, as in existential quantification. It is symbolised by '$(\exists x)$' or '$\exists x$', in which '$\exists$' denotes a variable binding operator.

2. "Existence" of an individual, to refer to the existence of a referent for a descriptive phrase. It is symbolised by '$\mathrm{E!}(\imath x)(\phi x)$', in which '$\mathrm{E!}$' denotes a predicate with $(\imath x)(\phi x)$ as subject, defined by (13.4):

$$\mathrm{E!}(\imath x)(\phi x) .\ =:(\exists b):\phi x .\equiv_x . x=b \quad \text{Df.} \tag{13.5}$$

[1] For discussions of the historical context of Russell's theory, see Butler *1954a*, Jager *1960a*, and *1972a*, ch. 5, Kaplan *1967a*, Vuillemin *1968a*, 73–78 and 272–273, Welding *1971a*, and Lambert and van Fraassen *1972a*, 152–157.

[2] Klemke *1970a* contains a reasonably representative selection of later views on descriptions.

[3] Russell stressed the importance of contextual definitions to Wiener (see my *1975a*, sect. 8). See also his *1905c*.

[4] The phraseology of Russell *1908a*, 253 (*1956a*, 93; van Heijenoort *1967a*, 176); circumflex added. Compare *PM*, vol. 1, 30.

[5] 'Meaningless' in the sense of not being well-formed. Russell also used this word to refer to the property that a propositional function has no truth-value, and to the case where a descriptive expression has no referent.

3. Existence of a class u, as in existential quantification over variables of higher order than individuals (case 1). It is symbolised by '$(\exists u)$' or '$\exists u$', in which '$\exists$' is a variable binding operator. This is also the sense of existence in the existential quantification of propositional functions ϕ, symbolised '$(\exists \phi)$', and of propositions '$(\exists p)$'.

4. Existence of a class u in the sense involved in Jourdain's '$\sim \mathrm{E}u$' on page 66 above. This sense seems to be associated with class abstraction, in which case it should be defined not from (13.1) but by the property that a propositional function $\phi\hat{x}$ exists (in the sense of case 3) to determine u. Thus it is a predicate with u as subject, and an appropriate definition might be

$$\mathrm{E}u \,.\, = \,.\, (\exists \phi)(x \varepsilon u \,.\, \equiv_x \,.\, \phi x) \quad \text{Df.} \tag{13.6}$$

5. "Existence" of a class u, to refer to its non-emptiness. It is symbolised variously as '$\exists ! u$', '$\exists u$' or '$\exists ! u$', in which '$\exists !$' or its variants denotes a predicate with u as subject, defined by

$$\exists ! u \,.\, = \,.\, (\exists x)(x \varepsilon u) \quad \text{Df.} \tag{13.7}$$

As well as various senses of the word 'existence', we also find a number of uses of the symbol '$\exists$'. Some of these senses of both word and symbol clash with others while some have connections. We saw on pages 15–16 above reason for thinking that Russell associated the existence of classes with their "existence" in connection with irrational numbers. We also noted there that in *Principia Mathematica* classes do not exist but will "exist" if not empty, while by contrast in a theory assuming classes the empty class exists but does not "exist". In addition we have the sense involved in Jourdain's 'E', which he associated with his claims that the "inconsistent" class Nc'No (page 67 above) and the multiplicative class $X'k$ of k (page 68 above) might "exist" though not exist in the sense of being abstractable. As defined by (13.6), this sense is equivalent to the membership of u to Cls. For

$$\mathrm{Cls} = \hat{u}\{(\exists \phi) \,.\, u = \hat{z}(\phi z)\} \quad \text{Df} \tag{13.8}$$

so that

$$u \varepsilon \mathrm{Cls} \,.\, \equiv \,.\, (\exists \phi) \,.\, u = \hat{z}(\phi z), \tag{13.9}$$

and it is easily proved that

$$u = \hat{z}(\phi z) : \equiv : x \varepsilon u \,.\, \equiv_x \,.\, \phi x. \tag{13.10}$$

Thus from (13.6) and (13.10) we have, as required :

$$\mathrm{E}u \,.\, \equiv \,.\, u \varepsilon \mathrm{Cls}.^{1} \tag{13.11}$$

[1] Compare *PM*, ✳20.4, ✳20.33 and ✳20.03 (where predicative functions are used because of the structure of type theory). Note also that Jourdain mentioned one side of this equivalence on p. 67 above, but mis-stated the equivalence on p. 69 above.

Further, if u exists ($\mathrm{E}u$) and $u\varepsilon v$, it follows only that v " exists " ($\exists!v$), not that it exists ($\mathrm{E}v$).[1]

The question of class abstraction was significant in 1905, since the status of classes and the means (if any) of their elimination was not then settled. By the time of *Principia Mathematica* classes were subsumed under propositional functions, and so the abstractional sense of existence was not discussed since it involved no additional ontological assumptions in that system. However, class abstraction there is not as free from assumption as Russell thought. Contextual definition is used in ✳20.01 :

$$f\{\hat{x}(\phi x)\} \,.\, = : (\exists \chi) : \chi!\hat{z} \,.\, \equiv_{z} .\, \phi z : f\{\chi!\hat{x}\} \quad \mathrm{Df}, \qquad (13.12)$$

where ' $\chi!\hat{x}$ ' denotes a ' predicative ' (that is, quantifier-free) function in type theory,[2] but ' $\hat{x}(\phi x)$ ' itself is a primitive notation. Russell might have done better to use his theory of descriptions to define directly

$$\hat{x}(\phi x) = (\iota u)(x\varepsilon u \,.\, \equiv_{x} .\, \phi x) \quad \mathrm{Df} \qquad (13.13)$$

without resort to contextual definition or primitive notation.[3] (13.13) does appear in ✳20.55 (note also vol. 1, 190), but as a theorem concerning classes, with ' = ' denoting class equality and not equality by definition.

This is one respect in which Russell did not present class abstraction and description, and their associated senses of existence, as clearly as he might have done. He also did not indicate the following connection. After defining the " existence " of an individual by (13.5) he stressed that, ' this sense of " existence " must not be confused ' with the " existence " of a class, as defined by (13.6) (vol. 1, 216). But he did not add that they are connected as follows. If $\mathrm{E}!(\iota x)(\phi x)$, then there is just one x satisfying ϕx, so that $\hat{x}(\phi x)$ is not empty and therefore " exists ". Hence we have

$$\mathrm{E}!(\iota x)(\phi x) \,.\, \supset .\, \exists!\hat{x}(\phi x). \qquad (13.14)$$

The closest approximation seems to be

$$\mathrm{E}!(\iota x)(\phi x) \,.\, \supset .\, (\exists x) \,.\, \phi x \qquad (13.15)$$

(✳14.201 ; see also vol. 1, 36 and ✳53.3).

The problems which we have seen Russell tackling in this section are delicate, and are hampered as much as helped by language. Russell

[1] I shall not explore the relationship between these ideas and Quine's philosophy of quantification and variablehood (see, for example, his *1969a*, *passim*). Perhaps ' E ' relates to *Elem* (see p. 36 above).

[2] This is the sense of ' predicative ' presented in *PM*, vol. 1, 161 (with p. 92), but on pp. 51–52 quantification over individuals seems to be allowed.

[3] Compare Quine *1941a*, art. 5 ; ' $\exists!\hat{x}(\phi x)$ ' would have to be assumed in later exegesis.

was aware of his predicament in this and indeed all his problems, for he concluded his letter of 13 January with the following answer to a request from Jourdain for more published work :

> I would publish my views as I get them, if they were not so liable to change and so full of errors which I keep on discovering. But I don't like publishing things which I see to be wrong before they are in print.

14. *January–March 1906 : Russell's substitutional theory of classes and relations*

We saw on page 56 above that Russell claimed the previous November that he was able to ' deal satisfactorily ' with the contradictions, and in the last section we found his new theory of definite descriptions begin to play a role in his logic. Now we can discover how large a role it played for a time, for we shall discuss in some detail this new method, which he called the ' no-classes ' (or ' no-class ') theory.

The accounts that Russell published at this time of his ' no-classes ' theory are very sketchy, and give the impression that it was an adjunct to, or variant of, some version of type theory.[1] The correspondence with Jourdain sheds a little more light, but the full picture is revealed only by certain manuscripts in the Russell Archives. It is with reference to these documents that the commentaries below, summarised from my *1974a*, 389–401, are based.

The most important point is that, ironically, the phrase ' the no-classes theory ' is not a definite description in Russell's sense, for it actually refers to a number of different though related theories. But to one of them he attached the significance and aspirations which he had reported to Jourdain, and this one he usually called ' the substitutional theory '. The central idea of this theory was the abandonment not only of classes and relations but also of propositional functions and free variables. This left only propositions and their truth-values, and individuals named by constants.

A basic component of the theory was called a ' matrix '. Written ' p/a ', it was neither a proposition p nor an individual a but *a denoting phrase in the sense of Russell's new theory of definite descriptions*, serving as a replacement for classes and relations by describing the operation ' the result of substituting for a in p by '. If x were substituted for a

[1] See Russell *1906a*, 45–47, and *1906c*, 636–638 (*1973a*, 154–156 and 200–204 respectively). A paragraph on it also intruded into *1908a*, 238–239 (*1956a*, 77 ; van Heijenoort *1967a*, 164–165).

in p, the result would be a proposition q. This process was symbolised by '$p/a{:}x{:}q$' or '$p\Big/\frac{x}{a}\,!q$', to be read as the proposition 'q results from p by substituting x for a in all those places (if any) where a occurs in p'.

The use of 'x' here motivates an important historical point. In his earlier attempts to solve the paradoxes, Russell had used 'x' as a free variable, and in the other theories which he called 'no-classes' he continued this practice and operated substitution and free variable techniques *simultaneously*. Only in this substitutional theory were free variables and propositional functions completely eliminated, leaving a residue of notations such as 'x' (now denoting a fixed if unknown individual) and terms such as 'variable' (as applied to a and p, and now referring to their capacity to take constants and propositions as substituents rather than ranging free in the orthodox sense).

Quantification is a good example of the new use of old notations. '$(x)\,.\,\phi x$' means that 'ϕx is true for all values of x'; but 'ϕx' cannot refer now to a propositional function in the normal sense but to a proposition whose truth-value depends on the (quite possibly unknown) individual x. Russell interpreted quantification in a way which is somewhat similar to the modern substitutional interpretation of quantification, in speaking of constants rather than the individuals which the constants name, and in giving prominence to truth-values.[1] Some correspondences are quite explicit, such as defining the negation of p by the falsehood of p and admitting propositions as abstract nameable objects. But the modern approach carefully notes the distinction between theory and meta-theory of which Russell was basically unconscious.

With these few preliminaries, we can now take Russell's first account to Jourdain, given in a letter of 19 January 1906:

> Classes, relations, etc. are now for me all of them what I call *matrices*, of which the nature (omitting subtleties) is as follows: Let $p/a{:}x$ represent what p becomes when x is substituted for a in those places (if any) where a occurs in p. Similarly $p/(a, b){:}(x, y)$ substitutes x, y for a, b, etc. Then p/a or $p/(a, b)$ or $p/(a, b, c)$ or etc. (which are not entities, but mere operations) are *matrices*; p/a is a class, $p/(a, b)$ a *dual* relation, etc. Under certain circumstances a dual relation is a Cls^2. Thus if
>
> $$q\,.=.\,(x)\,.\sim p\frac{x}{a}\quad[\mathrm{Df}],$$

[1] This interpretation was initiated in papers such as Marcus *1962a*; a critical view is taken in Quine *1970a*, 91–94. For a study of Frege's approach to this interpretation, see Stevenson *1973a*.

then

$$q/(p, a)`(p', a') \,.=.\, (x) \,.\sim p'\frac{x}{a'} \quad \text{[Df.]}$$

Thus

$$0 = q/(p, a) \quad \text{[Df]}$$

and 0 is a relation of p to a, and the sort of relation which is a Cls^2.

One may use single letters for classes and relations, but if one does, these letters are only significant in certain contexts. E.g. $p/a`x$ requires that $x$ should be an entity; thus $p/a`(p/a)$ is meaningless; thus $u \varepsilon u$ is meaningless . . .[1]

$\exists \text{Nc}`u$ does not lose its meaning if $\text{Nc}`u$ is of the form $q/(p, a)$. Thus $\exists \text{Nc}`u$ is

$$(\exists p', a') \,.\, q/(p, a)`(p', a').$$

E.g. $\exists 0$ is

$$(\exists p', a') \left\{(x) \,.\sim\left(p\frac{x}{a}\right)\right\} / (p, a)`(p', a')$$

where p, a are constants, which may be chosen as we like.

This text hints at further important features. Russell wrote here, as in his more extended treatments, '(p, a)' is interchangeable with 'p/a'; but then 'p' and 'a' must occur in a formula only in the matricial form 'p/a'. In fact, he did not explicitly discriminate between propositional and individual substitutions, so that a proposition could formally be substituted for an individual or vice versa.

This last point is particularly significant for the type structure in the theory, to which Russell alluded in this passage. In order to distinguish it from the type theory of *Principia Mathematica* I shall again employ double quotation marks when referring to it, although Russell used the word 'type' without special indication. The "type" of a matrix is characterised by the number of its substitutional places, so that p/a is of a different "type" from $p/(a, b)$, and so on. But then we should surely distinguish between propositional and individual substitutional places, so that, for example, the two-place "type" splits up into sub-"types" of the forms $p/(a, b)$ (corresponding to a two-place relation, as Russell mentioned above), $q/(p, a)$ (corresponding

[1] Russell changed topic here to object to a recent attempt in Peano *1905–08a*, 135, as reported by Jourdain, to defend his definition by abstraction. Russell retorted: 'Peano's improvement of his definition by abstraction seems a useless complication. If Num u is to mean any one of a set of objects, it is surely better to define the class of such objects, say $N`u$. Then where Peano has $\phi(\text{Num } u)$ the proper thing is

$$\alpha \varepsilon N`u \,.\supset.\, \phi\alpha$$

where $\phi(\text{Num } u)$ is any proposition which is affirmed by Num u'.

Jourdain also mentioned a letter from Peano which has not survived but may be the one that he cited in *1917b*, 149–150.

'$\exists$' in the next sentence of Russell's letter refers to "existence".

to a class of classes if p/a corresponds to a class, as he also mentioned), and $q/(p, p')$. The definition of 0, given in the above passage by

$$0 = \left\{(x) \,.\, \sim p\,\frac{x}{a}\right\}\Big/(p, a) \quad \text{Df}, \tag{14.1}$$

applies only in the sub-" type " of form $q/(p, a)$. (14.1) does reflect this point, but only implicitly.

Jourdain does not seem to have appreciated the significance of Russell's new theory, for he did not write again until 14 March 1906, when he asked a number of questions concerning Russell's recently published reply *1906a* to Hobson. This paper contained one of Russell's brief sketches of a ' no-classes ' theory, and Jourdain enquired of the status of the classes W and Cls in the substitutional theory. Russell's answering letter of the next day contained this explanation :

> ' $u\varepsilon$Cls ' is not *nonsense* on my present view, only the u should be replaced by p/a, where p/a is a ' matrix '. I use Greek letters for ' matrices ', Latin letters for entities ; thus
>
> $$\alpha\varepsilon\text{Cls} \,.\, = \,.\, (\exists p, a) \,.\, \alpha = p/a \quad \text{Df}$$
>
> (roughly speaking). And
>
> $$\alpha = p/a \,.\, = \,.\, \alpha`x \equiv_x \,.\, p/a`x \quad \text{Df},$$
>
> where $p/a`x$ means the same as $p\,\frac{x}{a}$. α is nothing but a typographical abbreviation for something of the form q/b ; it has no more reality that [sic] $\frac{d}{dx}$ or ∇^2. But symbolically, my no-classes theory makes very little difference—almost everything remains much as it was, but with a new interpretation of the symbols.
>
> W has now become wholly indefinable, for this reason, that, if we call a *type* such a thing as Cls^2, Cls^3, etc., the existence-theorems for segments of W require us to go further and further up the hierarchy of types, and no type will give the whole series, because a higher type would always give more. Now a series is meaningless unless all its terms are of the same type ; and there is no such thing as Cls^∞ or Cls^ω.[1] Hence there is no such thing as the whole series of ordinals. This is the solution of Burali-Forti [*1897a*, art. 10].

[1] This letter appeared as a little centennial tribute to Russell in my *1972a*, and here is a suitable point to correct a small error on p. 107. ' Cls^n ' refers to the class of . . . of classes, and not, of course, to the class of ordered n-tuples. Russell was denying transfinite type levels (on whose construction see Scholz and Hasenjäger *1961a*, 398–430, and Andrews *1965a*), though he fleetingly admitted them in *1903a*, art. 498. Similarly, he allowed the possibility of constructing ω_ω and $\aleph_\omega$ only in *1908a*, 261 (*1956a*, 101 ; van Heijenoort *1967a*, 182) ; compare *1903a*, art. 300.

The most extended section of this letter dealt with the prehistory of the substitutional theory. Jourdain had asked Russell : ' About when did you first have the idea that not every propositional function determines a class, and was it in consequence of *your* contradiction and not Burali-Forti's ? ' He received this wonderful answer :

> I am not sure that I can remember how my ideas developed. But I will tell you all I can. You will see that in my book [*The Principles*] (p. 104, art. 104) I suggest that certain functions do not determine a *class as one*. This is practically the same doctrine as that they do not determine a class, for a *class as many* is not an entity. (By the way, the view I now adopt, that a propositional function must not be varied, is discussed on p. 103, second par. of art. 103, and rejected as making mathematics unworkable. I have now discovered how, by substitutions, to work mathematics with this view.) My book gives you all my ideas down to the end of 1902 : the doctrine of types (which in *practice* is almost exactly like my present view)[1] was the latest of them. Then in 1903 I started on Frege's theory that two non-equivalent functions may determine the same class ;[2] also I considered whether one should distinguish the two definitions
>
> $$x\varepsilon_1 u \,.\, = \,.\, (\exists\phi) \,.\, u = \hat{z}(\phi z) \,.\, \phi x \quad \text{Df}$$
>
> $$x\varepsilon_2 u \,.\, = :(\exists\phi) \,.\, u = \hat{z}(\phi z) : u = \hat{z}(\psi z) \,.\, \supset_{\psi} \,.\, \psi x \quad \text{Df.}$$
>
> But I soon came to the conclusion this wouldn't do.[3] Then, in May 1903, I thought I had solved the whole thing by denying classes altogether ; I still kept propositional functions, and made ϕ do duty for $\hat{z}(\phi z)$. I treated ϕ as an entity. All went well till I came to consider the function W, where
>
> $$W(\phi) \,.\, \equiv_{\phi} \,.\, \sim\phi(\phi).$$
>
> This brought back the contradiction, and showed that I had gained nothing by rejecting classes.

[1] There is an important difference, however ; for now there is no single type of propositions (compare Russell *1903a*, art. 500).

[2] See the appendix to Frege's *1903a*, where he proposed that two concepts with the same extension are equivalent if and only if each extension does not fall as an object under the other. Russell discussed this approach in his notes on Frege, and also on ff. 123–124 of the undated manuscript ' General theory of classes ' (the one of two with this title numbered 230.030940). He does not seem to have found the formal inadequacy of Frege's approach which was demonstrated by Lesniewski (see Sobocinski *1950a*, 220–228) or Quine *1955a*.

[3] See the undated manuscript ' On meaning and denotation ', esp. ff. 83–87.

The latter part of 1903 and the beginning of 1904 I spent on the Fiscal Question. Then in April 1904 I began working at the Contradiction again, and continued at it, with few intermissions, till January 1905. I was throughout much occupied by the question of Denoting, which I thought was probably relevant, as it proved to be. A *denoting* function is, broadly, any function which is not propositional ; at times I have used $\phi`x$ for a denoting function and $\phi!x$ for a propositional function. The first thing I discovered in 1904 was that the variable denoting function is to be deduced from the variable propositional function, and is not to be taken as an indefinable.[1] I tried to do without ι as an indefinable, but failed ;[2] my success later, in the article ' on Denoting ', was the source of all my subsequent progress. Most of the year, I adhered to the ' zig-zag ' theory,[3] and worked at different sets of primitive propositions as to what functions determine classes. But I never got a set of primitive propositions that would really work, and all the sets were horribly complicated and un-obvious. I soon discovered that the difficulty comes only where ' all values of ϕ ' are concerned, and I thought perhaps this was due to a vicious circle, as follows : if

$$\psi x \,.\, = \,.\, (\phi) \,.\, f(\phi, x) \quad \text{Df},$$

it is part of the meaning of ψx to assert $f(\psi, x)$; thus ψ asserts something which cannot be defined till ψ is defined, and which is yet presupposed in the definition of ϕ.[4] Gradually I discovered that to assume a separable ϕ in ϕx is just the same, essentially, as to assume a class defined by ϕx, and that non-predicative functions must not be analysable into a ϕ and an x.[5]

About June 1904, I tried hard to construct a substitutional theory more or less like my present theory. But I failed for want of the

[1] There are many manuscripts of this period on denoting. Very briefly, some show denoting and propositional functions used in harness, and some extensively discuss denoting complexes. See especially ' On meaning and denotation ' (undated), ' On the nature of functions ' (1904), ' On the functionality of denoting complexes ' (November 1904), ' On functions, classes and relations ' (1904), and ' On fundamentals ' (1905), esp. ff. 18–81, on which Russell noted that the ideas developed there led him to the theory presented in ' On denoting '.

[2] See the undated manuscript ' On meaning and denotation ', esp. ff. 87–91.

[3] Russell's name for theories which avoid ' too complicated ' propositional functions ; see his *1906a*, 38–43 (*1973a*, 145–151).

[4] Note Russell's interest in vicious circles, and compare p. 66 above. On the specific point here, see the worksheets ' FN ', f. 564.

[5] See the manuscripts ' On functions ' (1904), ' Plan for a general introduction to Vol. II ' (1904) *passim*, and ' FN ' *passim*. ' Non-predicative ' here describes propositional functions which do not determine classes (see Russell *1906a*, 34 (*1973a*, 141)), as opposed to its meaning in *Principia Mathematica*.

theory of denoting : also I did not distinguish between *substitution* of a constant for a constant and *determination* of a variable as this or that constant.[1] Hence I abandoned the attempt to get on by means of substitution.

In 1905 for the first time I worked seriously at Burali-Forti's contradiction. I had never paid much attention to it before, because it was so much more complicated than mine that it seemed likely either to be soluble in some purely technical way, or to be not soluble until mine had been solved. I give a brief account of the results in Section I of my London Mathematical Society paper.[2] Then I tried a theory that functions are non-predicative when the same variable occurs in positions of different *type*, i.e. as in $x \varepsilon x$, in a position appropriate to an individual and again in a position appropriate to a class (or to a relation, or etc.). This excluded the right cases, but was hard to work technically.[3] My present theory shows why it excluded the right cases, and absorbs whatever was right in it.

Then, last autumn, as a consequence of the new theory of denoting, I found at last that substitution would work, and all went swimmingly.

Jourdain also asked about the relationship between Zermelo's axiom of choice and the multiplicative axiom, and obtained the following information from Russell :

As for the multiplicative axiom, I came on it so to speak by chance. Whitehead and I make alternate recensions of the various parts of our book, each correcting the last recension made by the other. In going over one of his recensions, which contained a proof of the multiplicative axiom, I found that the previous proposition used in the proof had surreptitiously assumed the axiom. This happened in the summer of 1904. At first I thought probably a proof could easily be found ; but gradually I saw that, if there is a proof, it must be very recondite.

This last piece of information corroborates the earlier impression that Russell had come, albeit reluctantly, to realise the possible need for an axiom in this kind of analysis *before* Zermelo's famous paper was

[1] Russell is alluding to the mixed free variables/substitution kind of theory mentioned on p. 75 above.

[2] See Russell *1906a*, 34–36 (*1973a*, 141–144). The ' results ' were a generalisation of Burali-Forti's paradox rather than a solution of it.

[3] The manuscript ' On the functional theory of propositions and relations ' (1903) seems related to this approach.

published late in 1904.[1] His next remark indicates a change in his view of the relationship between his and Zermelo's axioms. We recall from page 63 above that he had accepted that his axiom was implied by Zermelo's. Jourdain suggested in his letter of 14 March that the multiplicative axiom could be recast in a form from which Zermelo's axiom could be deduced. He also continued the discussion of pages 68–69 above on class abstraction in this context :

Zermelo's axiom :

$$w\varepsilon\mathrm{Cls} \,.\, \supset : \dot{\exists} R\{R\varepsilon\mathrm{Nc}\to 1 : \mathrm{D}`R = \mathrm{Cls}`w \sim \iota\Lambda :$$

$$uRx \,.\, \supset_{u,x} \,.\, x\varepsilon u\} \quad \mathrm{Pp}^{2}$$

(the form

$$\{R\varepsilon\mathrm{Nc}\to 1 : \mathrm{D}`R = \mathrm{Cls}`w \sim \iota\Lambda : uRx \,.\, \supset_{u,x} \,.\, x\varepsilon u\} \sim = _{R} \Lambda$$

(is not false for every R) seems preferable, for it does not assume that all such R's form a class).

You contrast (*1906a*, 49 [*1907a*, 159] this with the multiplicative axiom ; Zermelo's form cannot be deduced from it.

Now, it seems to me that Zermelo's axiom can be deduced from this :

$$k\varepsilon\mathrm{Cls}^{2} : x\varepsilon k \,.\, \supset_{x} \,.\, x \sim = \Lambda :. \supset :$$

$$\{u\varepsilon\mathrm{Cls} : u \subset \mathrm{U}`k : z\varepsilon k \,.\, \supset_{z} \,.\, z \cap u\varepsilon 1\} \sim = _{u} \Lambda.^{3} \qquad (1)$$

(I *think* this is equivalent to :

$$\exists u : u \subset \mathrm{U}`k : z\varepsilon k \,.\, \supset_{z} \,.\, z \cap u\varepsilon 1$$

and *not* to

$$\exists \dot{u}\{u \subset \mathrm{U}`k : z\varepsilon k \,.\, \supset_{z} \,.\, z \cap u\varepsilon 1\},$$

is not it ? Former does not imply all u's form a class : latter does.)

For if $w\varepsilon\mathrm{Cls}$ and $k = \mathrm{Cls}`w \sim \iota\Lambda$, we can *well-order by merely using (1)*.

Let u be the class which there *is* by (1),[4] then

$$w\varepsilon k \,.\, \supset \,.\, w \cap u\varepsilon 1$$

[1] Zermelo *1904a*. Compare Russell's remark in *1911a*, 32 (p. 171 below). During 1906 he wrote ' Multiplicative axiom ', his most substantial surviving manuscript on the subject. Just as Russell came to an axiom of choice not later than Zermelo, so Zermelo had Russell's paradox before Russell (see Hilbert's letter to Frege quoted in Thiel *1972a*, 94–95).

[2] ' $\dot{\exists} R$ ' means that the class of relations satisfying the condition expressed thereafter is not empty. We recall that Jourdain used ' $\sim$ ' for class complementation as well as for negation.

[3] Jourdain added the footnote : ' $R \sim \varepsilon \mathrm{Cls}^{2}$ excl necessarily ; Whitehead's axiom is a special case of [the] above (where $R\varepsilon\mathrm{Cls}^{2}$ excl) '. The form of expression using ' Λ ' at the end of this sentence follows Peano.

[4] An unhappy phrase, in view of the universal quantification of u in (1).

Let x be this single member. Then

$$w \sim \iota x \varepsilon k \,.\, w' = w \sim \iota x : \supset \,.\, w' \cap u \varepsilon 1.$$

Let x' be this member. Then

$$w' \sim \iota x' \varepsilon k \,.\, w'' = w' \sim \iota x' : \supset \,.\, w'' \cap u \varepsilon 1.$$

And so on, we order w in the *uniquely* determined series x, x', x'', ... $x^{(\omega)}$, $x^{(\omega+1)}$, ..., and we can easily prove that this series exhausts w, by the same process as Zermelo does.

And

$$w \varepsilon \mathrm{Cls} \,.\, k = \mathrm{Cls}`w \sim \iota\Lambda \,.\, (1) : \supset .$$

w can be well-ordered . ⊃. Zermelo's axiom holds for w (compare your *1906a*, 51 [*1973a*, 161]).

Russell accepted this argument, and stressed the restriction of the multiplicative axiom to classes of mutually exclusive classes: ' I think the axiom you suggest *is* equivalent to Zermelo's; what I call the *multiplicative* axiom has $k \varepsilon \mathrm{Cls}^2$ excl in the hypothesis '. However, examination of Jourdain's (1) reveals a number of difficulties. The first part of the axiom states that k is a class of non-empty classes; but ' k ' is left free, whereas any axiom of choice is supposed to apply to *every* class of classes and so should contain a universal quantifier over ' k '. The second part of the axiom seems to say that every sub-class u of $\mathrm{U}`k$ whose intersection with each member z of k forms a unit class is not empty; but the expression $\mathscr{A}$ within the curled brackets does not define a class, and so should not be followed by a statement of its non-emptiness. In fact, since u is claimed to intersect with each z in a unit class, the statement of non-emptiness of u seems redundant anyway. Further, this condition in $\mathscr{A}$ is already illegitimate under the structure of k; for if, for example, k contains $\iota`x$ and $\iota`y$ then x and y will have to be the common members of their respective classes with u, thus preventing u from having the required unit class intersection with any class α containing both x and y, and hence similarly prohibiting k from containing any such classes α.[1] Perhaps this fault can be repaired by requiring only that $z \cap u$ is a non-empty sub-class of $\mathrm{U}`k$ (' Cl ex$`\mathrm{U}`k$ ' in the notation of *Principia Mathematica*, ✳60.21). Finally, Jourdain's attempt to avoid the assumption that the choice classes u form a class causes him to quantify ' u ' universally, whereas axioms of choice assert only the existence of (at least) one choice class and so need an existential quantifier for ' u ' across $\mathscr{A}$ (or some equivalent formulation). Maybe

[1] I owe this counter-example to G. H. Moore.

. he wanted to state something like this :

$$k\varepsilon\mathrm{Cls}^2 : x\varepsilon k \,.\, \supset_x \,.\, x\sim=\Lambda :. \supset_k : \qquad (14.2)$$

$$(\exists u) : z\varepsilon k \,.\, \supset_z \,.\, z\cap u\varepsilon\mathrm{Cl}\ \mathrm{ex}'\mathrm{U}'k.$$

He was certainly lucky to avoid criticism of his axiom, for not only did he present it to Russell in this letter but he also published verbal forms in *1906b*, 283, *1906f*, 68–69, and *1907a*, 364.

In his letter Russell also referred to the following difficulty that he had mentioned in his reply to Hobson (*1906a*, 44 (*1973a*, 156)) of constructing ordinals as high as ω_1 in the substitutional theory. He saw numbers up to ω and $\aleph_0$ as definable in any " type " (or more strictly, in some sub-" types ", and assuming an infinity of individuals). However, higher transfinite numbers were restricted to certain " types ", because their definitions involved classes of smaller numbers. Thus, for example, ω_1 was an ' ordinal–ordinal ' (the ordinal of the series of ordinals of the second number-class), while $\aleph_1$ was similarly an ' ordinal–cardinal '. Again, ω_2 was an ' ordinal–ordinal–ordinal ' (the ordinal of a series of ordinal–ordinals), $\aleph_2$ an ' ordinal–ordinal–cardinal ', and so on, for a finite number of such constructions.[1] But Russell was uncertain how to execute these constructions, for reasons which he briefly outlined to Jourdain :

> ω_1 offers difficulties, because there is now no uniform method that I can discover of generating ordinals of the second class : the usual genetic method [by Cantor's first two principles of generation] breaks down, because it adds a relation or an ordinal number (which is a matrix) at the end of a lot of entities, which sins against the principle of homogeneity of type.

In his other questions Jourdain queried the validity of the $\aleph_0$-boots argument as an illustration of the multiplicative axiom, and suggested that the two definitions of infinity could be identified by means of his method of successive selections (described on page 57 above). Russell rejected both points, referring Jourdain on the latter to Pieri *1906a*.

The final point we take from this exchange is a personal matter. On the previous 28 December Jourdain had requested Russell's name as a reference ' with Hardy's and Whitehead's ' for the Allen Studentship, and Russell had agreed in his reply. Jourdain now reported that he

[1] Compare Russell *1973a*, 180–187. This line of reasoning appeared also in *1908a*, 260–261 (*1956a*, 100 ; van Heijenoort *1967a*, 181). Compare the (brief) treatment in *PM*, ✳265.

had won the Studentship and Russell replied : ' I am very glad indeed that you got the Allen : I had already heard the news from your sister [presumably Eleanor], whom I met the other day '.

On 21 March 1906 Jourdain wrote again, and complained for the second time that Russell had misrepresented his view of " inconsistent " classes such as the class W of all ordinals, in asserting that they were not classes at all.[1] Russell wrote back the next day in apology, and then returned to the substitutional theory, indicating how numbers were to be defined in higher " types " :

I am sorry to have mis-represented your view about the class of all ordinals. I was really considering *types* of views, and I ought perhaps to have omitted the reference to you. But I thought you distinguished consistent and inconsistent classes, allowing the latter to be still classes of some sort ; and I must have failed to realize that you regarded the class of all ordinals as a *consistent* class.[2] The theory you did advocate is a modification of the limitation-of-size theory : for of course this theory has innumerable forms according to the size which is held to be excessive. One might attach a meaning to $\mathrm{No}(\gamma)$, if No were a non-predicative function ; and this meaning might be read ' γ is an ordinal ', though $\gamma \varepsilon \mathrm{No}$ would be wrong.

My view at present is that every class, great and small, is merely a symbolic abbreviation ; as such, they are all legitimate if they have any significant definition ; but such forms as $\dot{x}(x \sim \varepsilon x)$ are not significant ; because one must have $x \varepsilon p/a$ or $x \sim \varepsilon p/a$ for significance, where we write

$$x \varepsilon p/a \,.\, = \,.\, p\,\frac{x}{a} \quad \text{Df.}$$

Thus $x \varepsilon x$ or $x \sim \varepsilon x$ is nonsense. Cls is a class of classes, and it is just as legitimate, in its proper context, as any other *matrix*. (I call a thing a *matrix* when it consists of a prototype and one or more initial subjects, so that it requires one or more arguments supplied to it to be significant. Thus in $p\,\frac{x}{a}$, p/a is the matrix ; in $p\,\frac{x, y}{a, b}$, $p/(a, b)$ is the matrix. These replace classes and relations.) Nc'Cls is all right, provided the Nc meant is what I call a *class-*

[1] Jourdain saw the misrepresentation in Russell's *1906a*, 44 (*1973a*, 153) ; compare the first complaint on p. 66 above. Jourdain published his complaint in *1906b*, 282, and *1922a*, 244.

[2] This is still wrong, since for Jourdain W is an " inconsistent " class.

cardinal (as opposed to an *entity*-cardinal), i.e. a cardinal applicable to a class of *classes*, not to a class of *entities*. Thus e.g.

$$p\,\frac{x}{a}\,.\equiv_x.\,x=c$$

(where c is fixed)[1] is satisfied by many values of p and a, but by only one value of p/a. Thus

$$\left(p\,\frac{x}{a}\,.\equiv_x.\,x=c\right)\Big/(p,\,a)$$

is a matrix determining *one* class; but this *one* is different from the entity-*one*, because p/a and p'/a' count as the same class if

$$p\,\frac{x}{a}\,.\equiv_x.\,p'\,\frac{x}{a'}.$$

In his long letter of 15 March Russell had told Jourdain that 'I may possibly be in Dorsetshire bicycling next month' and would try to visit Jourdain at Broadwindsor. Jourdain warned Russell two days later that 'the hills are very steep for cycling about here', but Russell was undeterred and promised on 7 April to spend the afternoon of 10 April with him before cycling on to Lyme Regis.[2] Unfortunately there is no record of their conversation, but when Jourdain wrote again to Russell he produced one of his most interesting contributions to their discussions.

15. *May–October 1906: Jourdain on ordinals, and Russell on propositions*

May 25, 1906.

Dear Russell,

Please tell me if there is anything wrong with these new nominal definitions of ordinals and cardinals which seems to me to be exactly what I want on the 'limitation of size' theory.

According to you, a *real* number is a certain class of *rationals*: your definition of a cardinal as a class of similar classes is also

[1] A definition of identity is assumed here. Russell muddled the definition in his substitutional theory by failing to distinguish between propositional and individual substitutions (see my *1974a*, 396). An appropriate definition here would be

$$x=c\,.=.\,(p)(p/c'x\,|\,p)\quad\text{Df.}$$

[2] On 11 and 12 April there was an exchange of brief notes on forwarding letters for Russell to Lyme Regis.

(what is necessary) nominal, but is subject to the difficulty (selon moi) that

$$u \varepsilon \mathrm{Cls} : \exists u : \supset_u . \mathrm{Nc}`u \text{ sim } x \exists (x = x)$$

(class of all entities).[1] This latter definition is unlike the former in that our cardinal is not a class of previously defined *numbers*.

Now, if we define it thus (as I do below) we *avoid* having our cardinals classes of too many things.

Although cardinals *can* be defined independently of ordinals, I begin, for shortness, with ordinals.

(1) Define Λ, $\iota\Lambda$.

(2) Definitions of series, etc. If we have terms such that one and only one of the relations $\succ$ ($a \succ b$ means '*a* follows *b*'), $=$, $\prec$ between any two of the terms, these terms form a '*series*'.

The term $\iota\Lambda$ can be proved to form a series. Define this series as the ordinal 1. $\iota\Lambda$ followed by 1 forms a 'series'.

$2 =$ series $\iota\Lambda$ followed by 1
$3 =$ series $\iota\Lambda$ followed by 1 followed by 2 } Dfs

and so on. We get, by induction, the 'finite ordinals'.

Now, in the order of these definitions, these finite ordinals form a *series*; for, if *n* and *m* are finite ordinals and $m \succ n$, *m* cannot also be a segment of *n*; and also the whole series is no segment of itself. Thus

$\omega =$ series 1, 2, . . . Df . . .

We then proceed to $\omega + 1$, . . .

Jourdain wrote his paper replying to Hobson's *1905a* around this time, and a passage in it may shed light on these rather unclear definitions: 'Now I see no reason for denying with Hobson, the existence of the series *W* (and the "field of the relation" *W*, the class *w* of all ordinal numbers) . . .' (Jourdain *1906b*, 267). He cited here art. 96 of Russell's *The Principles* for the definition of the field of a relation, but seems to have interpreted Russell's definition of the field as a class to be the definition of a *relation*. Thus for him a series is now a relation; for example, the series *W* of all ordinals has the unordered class *w* of all ordinals as its field.

Let us apply this interpretation to Jourdain's new definitions of ordinals. The sketched definition of a series shows that he followed Russell in defining a series to be a connected, asymmetrical (and presumably transitive) relation. But then a series cannot have a unit class as its field, nor can any of its terms be its own successor. Thus all

[1] We have another example here of '$\exists u$' meaning that *u* "exists" rather than denoting existential quantification over *u*. For Jourdain's criticism of Russell's definition, see p. 49 above.

the definitions are invalid. Had he replaced asymmetry with anti-symmetry in his definition of a series, then instead of assuming that

$$(x)(y)(\sim . xRy . yRx) \tag{15.1}$$

he would have adopted

$$(x)(y)(xRy . yRx : \supset . x=y), \tag{15.2}$$

and the desired properties would have been available.[1] Then he could have tried definitions of the following form :

$$\left.\begin{array}{ll} 0=\Lambda & \text{Df} \\ 1=0\prec 0 & \text{Df} \\ 2=(0\prec 0)\prec(0\prec 1)\prec(1\prec 1) & \text{Df} \\ 3=(0\prec 0)\prec(0\prec 1)\ldots\prec(1\prec 2)\prec(2\prec 2) & \text{Df} \\ \ldots\ldots & \end{array}\right\}, \tag{15.3}$$

where ' $\prec$ ' is his notation for ' is followed by ', but used under the revised definition of a series and applied also between propositions. However, further difficulties are waiting at the transfinite level which he might not have noticed, for he would have to assume not only the well-ordering principle but also the possibility of definition by transfinite induction.[2]

But these criticisms should not obliterate the fact that there is a significant element of novelty in Jourdain's definitions, and it is rather surprising that he gave them only one allusion (*1913a*, 493) in his published work. For in basic aim they relate to the definitions of ordinals proposed by von Neumann in his *1923a*. In Russell's notation von Neumann's definitions read :

$$\left.\begin{array}{ll} 0=\Lambda & \text{Df} \\ 1=\iota\text{`}0 & \text{Df} \\ 2=\iota\text{`}0\cup\iota\text{`}1 & \text{Df} \\ 3=\iota\text{`}0\cup\iota\text{`}1\cup\iota\text{`}2 & \text{Df} \\ \ldots\ldots & \\ \omega=\iota\text{`}0\cup\iota\text{`}1\cup\iota\text{`}2\cup\ldots & \text{Df} \\ \ldots\ldots & \end{array}\right\}, \tag{15.4}$$

[1] I am grateful for this suggestion to R. Bunn, who is preparing an extensive study (based on his *1974a*) of the foundations of set theory as developed from 1880 to 1930. The point made here could be applied also to Russell's definitions of ordinals, which exclude 1 (see *PM*, vol. 1, 377 ; and compare Quine *1941a*, 159 (*1966a*, 32)).

[2] In this form of definition a property is defined for a transfinite ordinal n if it is defined for 0 (say) and for all ordinals less than n. See, for example, *PM*, ✳257 ; and compare Cantor *1897a*, art. 18.

The problems of order which beset Jourdain's definitions are avoided here, since order is definable solely in terms of membership and inclusion :

$$m<n \;.\equiv.\; m\varepsilon n : m\leqslant n \;.\equiv.\; m\subset n.^{1} \qquad (15.5)$$

Further, von Neumann explicitly considered definition by transfinite induction, especially in his *1928a*.

In his letter Jourdain went on to define cardinals, and claimed to be able to prove various properties of cardinals and ordinals. These are his results, as he stated them :

> No series of ordinals (in order of magnitude) contains all of them.
>
> Any well-ordered series is like some series of ordinals.
>
> If $\zeta\varepsilon$No, [the] cardinal associated with ζ or
>
> $$C(\zeta)=\dot{\gamma}(\gamma<\zeta) \qquad \text{Df.} \quad [(15.6)]$$
> $$C(\xi)=C(\zeta) \;.=.\; \dot{\gamma}(\gamma<\xi)\ \text{sim}\ \dot{\gamma}(\gamma<\zeta) \qquad \text{Df.}$$
> $$C(\xi)<C(\zeta) \;.=.\; \text{a part of } \dot{\gamma}(\gamma<\xi)\ \text{sim}\ \dot{\gamma}(\gamma<\zeta). \quad \text{no part of } \dot{\gamma}(\gamma<\xi)\ \text{sim}\ \dot{\gamma}(\gamma<\zeta) \qquad \text{Df.}$$
>
> If $u\varepsilon$Cls, and there is an ordinal ζ such that
>
> $$u\ \text{sim}\ \dot{\gamma}(\gamma<\zeta)$$
>
> then we define
>
> $$\text{Nc}'u=C(\zeta) \quad \text{Df.}$$

Since Jourdain's definition of a series is inappropriate, all his subsequent definitions will be invalid and his proofs of theorems at best only suggestive. In particular, the property of asymmetry embodied in his '$<$' means that his definition (15.6) of a cardinal is unacceptable, since it then follows that different transfinite ordinals cannot be associated with the same cardinal. Russell took up the definition of a cardinal in his reply of 14 June to Jourdain's proposals :

> I am sorry to have left your letter so long unanswered, but I have been very much occupied, and have not had leisure to consider it. Your suggestion is very interesting. I do not see any objection to it, provided you are content that a cardinal shall be meaningless

[1] Priority over von Neumann in descents of membership is due to D. Mirimanoff (see his *1917a*, esp. pp. 41–46, and *1917b*, 210–213), and also to Berry, who in a letter of 26 September 1908(?) to Russell (see pp. 50–51 above) outlined his theory of ' epsilonic ' classes. Such a class u_1 has the property that ' in the progression $u_1, u_2, u_3 \ldots$ we always have $u_{\gamma+1}\varepsilon u_\gamma$ '. Unfortunately we hear no more, but it seems possible that this creator of paradoxes could have obtained paradoxes of circular membership or of grounded classes. (Compare Mirimanoff *1917a*, Quine *1951a*, 128–129, and Yuting *1953a*.)

except as applied to a well-ordered class.[1] I would suggest the following emendation on its own basis : take as the Nc associated with an No ζ

$$\dot{\gamma}\{\gamma \prec \zeta \,.\, \dot{\beta}(\beta \prec \gamma) \sim \text{sim}\, \dot{\beta}(\beta \prec \zeta)\} \quad \text{Df.} \qquad [(15.7)]$$

Then all ordinals of the second [number-]class have the class of finite ordinals as their cardinal ; and so on. Or even you might take ω itself as the associated cardinal, putting as associated Nc

$$\text{Min}'\dot{\gamma}\{\dot{\beta}(\beta \prec \gamma)\ \text{sim}\ \dot{\beta}(\beta \prec \zeta)\} \quad \text{Df,} \qquad [(15.8)]$$

where Min stands for *minimum*.

I cannot, of course, adopt these definitions, because they depend upon the view that every genuine class can be well-ordered. But on that basis, I see nothing whatever to object to in your letter.

Russell's emendation to Jourdain's definition of cardinals strengthens the anticipation of von Neumann, who in his *1928a*, sect. 7 defined a cardinal as an ordinal which was not similar to any smaller ordinal. In a way Russell's two proposals meet von Neumann's requirement between them : (15.7) contains the property of non-similarity though it defines the cardinal in his own general style in terms of classes of (in this case, ordinals), while (15.8) defines a cardinal as ordinal, in terms of the property of ' minimising ' (presumably taking the smallest in order of) the ordinals satisfying the criterion of (15.8). It seems rather surprising that he did not object to the type-mixing character of Jourdain's cumulatively ranked definitions, for he continued this letter with an affirmation of belief in the substitutional theory, which had a type structure of its own :

> At the same time, the no-classes theory makes all such considerations superfluous, if it is right. For it shows that we can employ the *symbol* $\dot{x}(\phi x)$ without ever assuming that this symbol in isolation means anything. I feel more and more certain that this theory is right. In order, however, to solve the *Epimenides*, it is necessary to extend it to *general* propositions, i.e. to such as $(x) \,.\, \phi x$ and $(\exists x) \,.\, \phi x$. This I shall explain in my answer to Poincaré's article in the current Revue de Métaphysique.

The references to the liar paradox and to the reply to Poincaré are

[1] Presumably Russell had in mind here Cantor's definition of cardinals in *1895a* by a process of double abstraction from classes which are not necessarily well-ordered. But Cantor defined $\aleph_1$ in *1897a*, art. 16 as the cardinal of the (well-ordered) second number-class, and Russell's point does not seem strong.

so far as I know, is the first to distinguish between the assertion of *any* proposition of a certain form and the assertion of *all* such propositions. See *1893a*, art. 17 [first in *1879a*, art. 11].

If we want to state any case of ϕx, we write '$\vdash . \phi x$'; if we want to state that all cases are true, we write '$\vdash . (x) . \phi x$'. Thus there is to be no such thing as a statement that something holds of all propositions, though there are ambiguous statements applying to any propositions; but these do not state a new proposition. Thus

$$'p . \supset . p \vee q'$$

is not a new proposition over and above all its instances, though

$$'(p, q) : p . \supset . p \vee q'$$

would be a new proposition if it were a proposition at all.

There is something on the difference between '$\vdash . \phi x$' and '$\vdash . (x) . \phi x$' in ✳7 of my 'Theory of Implication' [*1906b*] in the American Journal.

Russell seems to admit proposition letters as free variables, but not susceptible to quantification. It would be tempting to think that he treated them as schematic letters and his axioms as schemata,[1] but such a view (both for this passage and for *Principia Mathematica*) can be maintained only because of the fact that he did not explicitly state any rule of inference for substitution and so in effect rather than in intention adopted the schematic viewpoint.

However, there is one respect in which a schematic approach does relate to Russell's current denial of propositional quantification. One of his reservations about the substitutional theory may have been its commitment to propositions as abstract objects. He was developing a correspondence theory of truth at this time, and he found it difficult to postulate the existence of objective falsehoods which false propositions could name or in which one could have a belief. In several manuscripts of this period he considered denying propositions as abstract objects in connection with both truth and no-classes theories, and also showed some awareness of the ontological implications of quantification. For example, he wrote in 1907: 'A value of an apparent variable must be something, and thus the no-classes theory won't work. It worked while we had propositions because then they became apparent variables where a variable matrix was wanted'.[2]

[1] The interpretation of axioms as schemata seems to be due to von Neumann *1927a*.

[2] 'Fundamentals' (1907), f. 6. See also 'What is truth?' (June 1905), 'On fundamentals' (1905), 'Logic in which propositions are not entities' (April–May 1906), 'The paradox of the liar' (September 1906), 'On the functional theory of propositions, classes and relations' (1906), and the miscellaneous sheets catalogued under 230.031000.

But on the whole he was vacillatory on such questions and did not fulfil the promise of his insights.[1] For example, this creator of the theory of descriptions, who preferred to say that there was not an entity to which a descriptive phrase referred rather than that there was a non-entity to which it had reference, did not take the analogous step and assert that there was not a fact to which a false proposition corresponded rather than worry about whether or not there was a non-fact which it could name. Again, while he denied objective falsehoods in his *Philosophical Essays* of 1910[2] and wrote in the first volume of *Principia Mathematica* of the same year that 'since we never have occasion, in practice, to consider propositions as apparent variables' (p. 129), he also assumed the abstract status of propositions in using propositional quantification as the basis of the stratification of truth-values (pp. 41–43) '$(p)\,.\,p$ is false' has an order of truth-value one order higher than that of any p which it mentions), and in his solution of the liar paradox (p. 62 : 'I am asserting a false proposition' is a proposition of a higher order).[3]

But this type theory is not the one to which Russell referred in the last extract, for there both propositional *and functional* quantification were denied. He had in mind another version of a 'no-classes' theory in which free variables and substitution were both used.[4] It is this theory to which he referred on 22 October in reply to an encouraging note from Jourdain of 10 October :

> I am glad you feel attracted by the no-classes theory. I am engaged at present in purging it of metaphysical elements as far as possible, with a view to getting the bare residuum on which its success depends.
>
> I decided not to publish the paper I read at the London Mathematical [Society] in May; there was much in it that wanted correction, and I preferred to wait till I had got things into a more final shape.

[1] Pace D. Lackey in Russell *1973a*, 134, who quotes another such passage (from 'The paradox of the liar' (1906), f. 106; for 'whatever' in the transcription read 'what') and says without qualification that Russell 'was certainly not confused' on such questions.

[2] Russell *1910b*, ch. 7, replacing the last part of *1907b*, where he had been unable to decide for or against objective falsehoods. Propositional quantification does not seem to have been prominent in mathematical logic until the equivalential calculus initiated in papers such as Tarski *1923a*.

[3] With propositional quantification thus admitted, he could speak of a 'propositional function' of propositions, in analogy with such functions of individuals and of functions—and he *did* do this in *PM*, part I, although he seemed to restrict this sense of the term to logical combinations of propositions, since their quantification was supposed to be being avoided !

[4] See especially the undated manuscripts 'On types', 'Notes on types' and 'General theory of functions', which seem to belong to this period, and also 'The paradox of the liar' (September 1906).

The paper which Russell mentioned was his principal manuscript on the substitutional theory, and its withdrawal marked the demise of the theory itself. Entitled ' On the substitutional theory of classes and relations ', it was submitted to the London Mathematical Society in May 1906. The first referee kept the paper for months, but eventually recommended it for publication in the Society's *Proceedings*. The second referee was the logician A. B. Kempe, who received it in October and quickly gave it a recommendation, although he was critical of certain unclarities. Russell then withdrew it, presumably for the accumulation of difficulties at which he hinted in the above passage to Jourdain.[1]

The substitutional theory is an exceptionally ingenious construction even by Russell's standards ; but it is flawed by the lack of a number of distinctions (such as that between propositional and individual substitutions), and the failure to clarify the status of some of its components as linguistic or abstract objects. However, it did not deserve his later description as ' all nonsense ' (*1967a*, 183), and is of considerable historical importance for the development of his thought. In fact, it solves the mystery of another of his recollections : that the discovery of the theory of denoting ' made it possible to see, in a general way, how a solution of the contradictions might be possible '.[2] In *Principia Mathematica* descriptions play a reasonably significant role ; they help to explicate the category of incomplete symbols and provide the apparatus of descriptive expressions. But they do not have the central position that this recollection suggests. For such a status for descriptions we must turn to the substitutional theory, with its basic component ' *p*/*a* ' acting as denoting phrase. In other words, *the substitutional theory is the missing-link between Russell's theory of denoting and* Principia Mathematica. But it fell soon after it rose, and remained only as a vague and regrettable episode in his memory when he came to think back years later about the period of his logical studies.

16. *January–February 1907 : Russell's philosophy of relations*

The next sign of correspondence is a note by Jourdain in his notebook of a letter dated 30 January, in which he queried Russell's treatment of Dedekind's theory of irrationals in *The Principles*, art. 266 (the first topic discussed in the surviving letters). We recall from pages 13–16 above that Russell's criticisms of his predecessors' work on irrationals

[1] For a fuller account of the withdrawal of the manuscript and of the supporting details to the judgements in the paragraph following, see my *1974a*, 398–401. The manuscript was published recently in Russell *1973a*, 165–189.

[2] Russell *1944a*, 14 ; see also *1967a*, 152 and 177, and *1959a*, 79.

was not entirely justified, and in his answer of 3 February he retracted his views on Dedekind to some extent:

> My criticism of Dedekind's axiom was merely to point out that in itself it does not discriminate between discrete and continuous series. E.g. an ω satisfies it as well as a θ. But I see on looking up what I said there, my words are ambiguous and are capable of a false meaning. Dedekind was of course only thinking of *compact* series, which are particularized by his axiom as he intends.

Russell then turned to a query by Jourdain as to whether his claim in *The Principles*, arts. 63 and 474 that all definitions are definitions of classes was not refuted by definitions of relations. Russell admitted that

> the dictum that all definitions are definitions of classes is certainly false. I was *meaning* ' no definitions are definitions of individuals '. But relations (e.g. identity) are just as definable as classes. Relations, however, being (in our calculus) relations in extension, may be regarded as classes of couples. Thus possibly one might stand up for the original saying in a sense. The truth, however, which is important, is that all definitions are definitions of propositional functions, in so far as they can be regarded as definitions which are complete in themselves.

In one of the places cited by Jourdain from *The Principles* (namely, art. 474) Russell asserted that ' a definition is always either the definition of a class, or the definition of the single member of a unit class . . . a definition can only be effected by assigning a property of the object or objects to be defined '. Presumably his comment to Jourdain was a stress that the single member of a unit class can only be defined *as* the single member and not as an individual (compare arts. 125–126 on unit classes and denoting).

The most important point raised by this extract is the interpretation of relations. One of Russell's earliest philosophical decisions was to adopt an *externalist* view of relations, under which the relation R which relates two objects x and y is itself separate from x and y (*1900a*, art. 10; compare *1901d*, 38–40, and *1903a*, arts. 212–216). This was opposed to the *internalist* view, in which R is regarded as built in to (x, y). From there Russell seems to have come to think of relations ' almost exclusively as *intensions* ' (to quote his later reminiscence in *1959a*, 87; compare *1903a*, arts. 28 and 98) and associated with properties, as opposed to the *extensional* view, where they are treated as classes

of ordered pairs. Although the issue of internality versus externality is independent of that between intensionality and extensionality, intensionality might have seemed to him to be the more suitable companion for externality. (The view that relations are both intensional and internal might be difficult to expound.) In *My Philosophical Development* he discussed these distinctions separately (*1959a*, 54–62 and 87–89), thus tending to suppress any relationship between them at all.

Russell's intensional view is confirmed in the last sentence of the passage above, where he stated that relations were subsumed under propositional functions. For he normally regarded propositional functions as linguistic expressions,[1] which could not be infinite classes of ordered pairs. But he also said that relations were ' relations in extension ', which contradicts this line of reasoning. The following passage from *Principia Mathematica* (vol. 1, 72) on the issue of intension versus extension may shed some light :

> Our theory of classes recognises and reconciles these two apparently opposite facts, by showing that an extension (which is the same as a class) is an incomplete symbol, whose use always acquires its meaning through a reference to intension.

This view is corroborated by a contextual definition of a relation in extension in ✳21.01, from which the calculus of relations is developed in analogy to the calculus of classes. Russell's desire to minimise the number of undefined terms in his system forbad him from assuming the ordered pair as primitive (when he could have reduced relational abstraction to class abstraction[2]). Instead he offered (in ✳55.01 with ✳35.04) its unit class ; namely, the following extensional definition of the ' ordinal couple ' of x and y, the relation with $\iota'x$ as domain and $\iota'y$ as converse domain :

$$x\downarrow y = \hat{v}\hat{w}\{v\in\iota'x \,.\, w\in\iota'y\} \quad \text{Df.} \qquad (16.1)$$

However, $x\downarrow y$ is defined *nominally* by (16.1) as the unit class of (x, y), in contravention of the method of defining relations in extension contextually.

Thus the relationship between intensions and extensions is not always

[1] See Russell *1903a*, art. 22 and ch. 7, *1905c*, *1912a*, 1–5 (*1956a*, 105–108), and *1913b*, 492 (*1973a*, 293). On Russell's treatment of intension and extension, see Peters *1963a*. Ramified type theory must assume intensions, for otherwise it would be vacuous.

[2] On this point see Quine *1941a*, art. 6. The definition of the ordered pair to reduce the logic of relations to the logic of classes was accomplished only after the publication of *Principia Mathematica* ; see Wiener *1914a*, Hausdorff *1914a*, 14, and Kuratowski *1921a*. Russell affirmed his intensional interpretation of relations to Wiener ; see my *1975a*, sect. 6.

clear in *Principia Mathematica*. However, Russell stressed the importance of propositional functions (which provide intensions) in a postcard of 12 February. Jourdain had asked him on 5 February to clarify a remark prefatory to the above extract from his letter, in which he had said that ' negative number, ratio, complex number, etc. are all classes, namely classes of relations '. In the postcard he corrected himself, and said that :

> Rationals, and numbers which have sign, *are* relations, not classes (unless a relation is taken to be a class of couples). It is a mistake to say that all definitions define classes—all define propositional functions, is the right remark.

17. *March–April 1907 : connections with Hausdorff, and cardinal exponentiation*

In his next letter, of 26 March, Jourdain changed the subject and returned to the equation

$$\omega_\beta = \beta \tag{17.1}$$

that he had studied in July 1904. We recall from pages 39–40 above that he had anticipated some work of Hessenberg and especially of Hausdorff in the construction of (17.1). Hausdorff's work was still to appear (though Hessenberg published his results in *1906a*, art. 85). Jourdain seems to have been unaware of Hessenberg's work, for he now described his own development of (17.1) :

> Consider the series of type ω
>
> $$0, 1, 2, 3, \ldots$$
>
> Replace each n by a series of type $\omega_{\omega_{\omega_{\cdots}}}$ to n terms (or $\omega_{(n)}$ for short).
>
> I am inclined to adopt, as an axiom, that there is no such thing as a $\omega_{(\omega)}$. If β is the type of W, we can prove that
>
> $$\omega_\beta = \beta ;$$
>
> a rather paradoxical result. Now
>
> $$\omega_{\omega_{(\omega)}} = \omega_{(\omega)}$$
>
> and $\omega_{(\omega)}$ is the *least* number like β. On account of this paradoxical property (though I cannot yet find any actual contradictions in $\omega_{(\omega)}$; e.g., that
>
> $$\omega_{(\omega)} \cdot \omega = \omega_{(\omega)},$$

whence would follow that

$$\alpha \times \beta = \alpha$$

when

$$\alpha = \omega_{(\omega)}, \quad \beta = \omega_{(\omega)} \cdot \omega),$$

I assume now the above axiom which tells us ' how far up the series of ordinals we may go ', and allows more of it than you do.

Hausdorff began publishing his theory late in 1907,[1] and a statement of some of his definitions and theorems may most conveniently provide the required comparison with Jourdain. An ordered class A is defined to be ' cofinal ' with an ordered sub-class M if no member of A succeeds every member of M in the ordering, and a positive ordinal is ' singular ' or ' regular ' according as it is or is not cofinal with a smaller ordinal. Two theorems state that the initial ordinal ω_β of a number-class is cofinal with β if β is a limit ordinal, and that an initial ordinal ω_γ is regular if γ is *not* a limit ordinal. It then follows that if ω_β is also to be regular, then it must *equal* β. But this gives us not only (17.1), which Jourdain felt to be ' rather paradoxical ', but also the ordinal $\omega_{(\omega)}$ which he had constructed to satisfy it.[2]

Jourdain does not seem to have published in detail his ideas on (17.1) and the ordinals which satisfy it.[3] Certainly he was in territory which type theory prevented Russell from entering, as Russell admitted in a reply of 11 April when questioning the illegitimacy of $\omega_{(\omega)}$:

> As to your axiom about ' how far up the series of ordinals we may go ', it seems to me not refutable, but rather arbitrary. I don't remember expressing myself about Cls^ω except in *1906c*, 637–639 [*1973a*, 201–203], and I have very little to say about it. Broadly, one can't proceed to the ω-th term unless the finite terms can be taken as one class ; now Cls, Cls^2, Cls^3, ... are all of different

[1] See Hausdorff *1907b*, 542 ; and also *1908a*, 440–444, and *1914a*, 86 and 129–133. His work seems based on Cantor's idea of the correlability of order-types in *1895a*, art. 10. His theorem on the cofinality of the ordinals up to ω_ω (inclusive) with the initial ordinals contained therein (*1914a*, 130) is similar to Jourdain's (9.2) on p. 39 above.

[2] These ideas relate to the theory of inaccessible numbers. See Bachmann *1955a*, ch. 7, Sierpinski *1958a*, 402–406, and Kuratowski and Mostowski *1968a*, 235–246 and 308–328.

[3] The closest allusion seems to be in Jourdain's *1906b*, 270–271. Some priority is due to Berry, who in a letter of 6 September 1905 to Russell (see pp. 50–51 above) reported seeing Jourdain's (17.1) (presumably in *1905a*, 53), but denied its paradoxical status. He called such ordinals ' self-suffixed ' and (converting to Hausdorff's terminology and Jourdain's notation) stated that for any ordinal γ a regular self-suffixed ordinal could be constructed, and that $\omega_{(\omega)}$ was the smallest of them. Jourdain had this latter result in the extract just quoted, while Hessenberg (*1906a*, art. 85) and Hausdorff (*1908a*, 443 ; compare *1914a*, 131) had the whole construction.

types, and therefore do not form a class. Hence Cls^{ω} seems to me meaningless. As to ω_{ω} and $\aleph_{\omega}$, I merely don't know of a satisfactory construction for them—I don't positively affirm that there is none.

Hausdorff's work was involved also in the second topic of this exchange, this time explicitly. Jourdain mentioned his method of defining the multiplication of an infinity of ordinals placed in inverse well-order, which he presented in his *1906e* as a generalisation of Hausdorff's definition in *1904a* of ordinal exponentiation. Russell alluded to it in reporting the progress of his own method :

> A similar definition applies to transfinite ordinal powers. I have adopted what I believe is substantially your definition of an infinite ordinal product and have worked through relation-arithmetic with it. It is on the whole very satisfactory.

Presumably Russell was describing the method presented in *Principia Mathematica*. It was based on Hausdorff's ' principle of first differences ',[1] but was also related to the method of intercalation which he had outlined on pages 19–20 above to Jourdain back in November 1902. Possibly he had been encouraged to pursue his own approach by the results that Hausdorff had found (apparently independently) in the meantime.

The last point that we take from Jourdain's letter of 26 March relates to the first in that it involves Cls^{ω} :

> A curious difficulty about any cardinal a^b, where b is transfinite, bothers me. If u is a class of cardinal a, and we replace each member of u by a class of a members, u becomes a class u' of cardinal $a \times a$, and $u' \varepsilon \mathrm{Cls}^2$. If we replace each u' by a class of a members, we get a class u'' ($\varepsilon \mathrm{Cls}^3$) of a^3 members, and so on (we can suppose the u's well-ordered and so avoid the multiplicative axiom).[2] Thus, to get a class of $a^{\aleph_0}$ members, in the above way, we must fabricate a class $u^{(\omega)}$ which $\varepsilon \mathrm{Cls}^{\omega}$. And these you don't admit.[3]

[1] Hausdorff developed his method in *1906a* and *1907a*, and used it also in *1907b* and *1908a* ; for a summary, see *1914a*, 147–158. His earlier papers were highly praised in *PM*, vol. 2, 391, where Russell presented his own definitions of ordinal multiplication in ✳172–✳173. In *1902a*, 31, ✳6.58, and *1903a*, art. 299, Russell had confessed his inability then to define the product of an infinity of ordinals.

[2] If a is finite then the axiom is not needed ; but if not, then surely the axiom is unavoidable in supposing the u's to be well-ordered.

[3] Jourdain now cited Russell's *1906c*, 637 and 639 (*1973a*, 201 and 203) but he might have recalled Russell's denial of Cls^{ω} from the previous March (see p. 77 above).

This method of exponentiation contrasts with Cantor's method in *1895a*, art. 4 (generalising his *1891a*) of forming the ' covering ' class of a class u with members of a class v. This class is the class of functions from u to v, and its cardinality is $\mathrm{Nc}`v^{\mathrm{Nc}`u}$. Jourdain had used the Cantorian approach in his *1905a*, 48–50, and in his *1922a*, 242 he stated it to be independent of axioms of choice. Thus this alternative should have seemed to be devoid of merit for him.

In his answering letter Russell reminded Jourdain of Cantor's method, and also described Whitehead's own approach using ordered couples of members chosen from each of the classes and then forming a multiplicative class, which was basically followed in ✱116 of *Principia Mathematica* :

> The way of defining exponentiation which you suggest is not the only way—there is e.g. Cantor's way, which gives a perfectly good definition of $2^{\aleph_0}$; or the following : Let $\breve{\alpha}`y$ represent all couples formed of y and a member of β [sic : read ' α '] ; then $\breve{\alpha}``\beta$ represents the class of all such classes of couples by choosing different members of $\beta$ as our $y$. Then $X`\breve{\alpha}``\beta$ (i.e. the multiplicative class of $\breve{\alpha}``\beta$) can be easily shown to exist, and
>
> $$\mathrm{Nc}`X`\breve{\alpha}``\beta = \mathrm{Nc}`\alpha^{\mathrm{Nc}`\beta}.$$

Jourdain still did not seem to understand, for when he wrote back to Russell the next day (12 April), he doubted the legitimacy of Russell's method of cardinal exponentiation :

> I presume, then, that (even if the multiplicative axiom could be proved), you would not admit the notion of a product of an infinity of cardinals. For this *must*, it seems, involve the notion of a Cls^{∞}.

When Russell found time on 26 May to reply, he rejected any such suggestion for Whitehead's definition of the multiplication of an infinity of cardinals. He also reported that he could now avoid the restriction of the definition to classes of mutually exclusive classes :

> Whitehead's definition involves no Cls^{∞}. I have made a slight improvement which avoids the necessity of the classes being mutually exclusive, namely
>
> $$\mathrm{Sel}`\kappa = 1 \to \mathrm{Cls} \cap \mathrm{Rl}`\varepsilon \cap \overleftarrow{\cup}`\kappa \quad \mathrm{Df}.$$

The notations here have the following meanings :

$1 \to \mathrm{Cls}$ is what was $1 \to \mathrm{Nc}$.

$$\mathrm{Rl}`\varepsilon = \hat{R}(R \subseteq \varepsilon) \quad [\mathrm{Df}],$$

which is a particular case of

$$\mathrm{Rl}'S = \hat{R}(R \subset S) \quad \text{Df.}$$

(I use $\hat{R}$ instead of $\dot{R}$ now.)[1]

$\overleftarrow{\mathrm{Cl}}'\kappa$ = relations whose converse domain is κ. This comes from

$\mathrm{Cl}'R$ = converse domain of R.

$$\overleftarrow{P}'x = \hat{y}(xPy) \quad \text{Df,}$$

and generally

$$Q'x = (\gamma y)(yQx) \quad \text{Df.}$$

Sel'κ stands for ' selections of κ '. Nc'Sel'κ is the product of the Nc's of κ's.

So Sel'κ is the class of choice classes for the class of classes κ, constructed via the class of one-many membership relations from members to sub-classes of κ and assumed via the multiplicative axiom to be non-empty ; and its cardinality is the required product of the cardinalities of the member classes of κ. Russell published a proof that this axiom implied (and thus was equivalent to) Zermelo's axiom in his paper of the following year (1908) outlining the system of *Principia Mathematica*,[2] and he developed this approach to cardinal multiplication in detail (though with a rather different notation) in that work.[3] He did not mention in his letter Jourdain's proposed axiom of March 1906 (page 81 above) ; perhaps by now he had noticed its unsatisfactory form.

Jourdain's letter had ended with a request for an offprint, and Russell obliged : ' I enclose " The Existential Import of Propositions ". It is amazing how MacColl's reply ignores all the points I have raised '. This statement heralds a change in the correspondence back to the more philosophical aspects of logicism from the mathematical technicalities of late.

[1] We can trace Russell's changes in notation for class abstraction. Until about April 1905 he used Peano's ' ɜ ' (for ' such that ') but then changed to Frege's apostrophe (see p. 46 above). Now he has adopted the circumflex, which he had been using since June 1904 for functional abstraction (see p. 36 above).

[2] Russell *1908a*, 259 (*1956a*, 98 ; van Heijenoort *1967a*, 180, where in the definition of $Z'\beta$, ' $\exists$ ' should be read for ' E ' and the defining condition of R should begin with ' $(\exists S)$. $Se\gamma$ '). *1908a* was written at this time (see p. 111 below). Compare *PM*, vol. 1, 388 (where the axiom is heralded), ✳85.63 and ✳88.33.

[3] *PM*, ✳114. With regard to notation, ' Sel'κ ' was replaced by ' ε_{Δ}'κ ' because the expanded treatment of selections allowed for selections from any relation (see ✳80.11), and ' Prod'κ ' denoted the multiplicative class of κ (✳115.01). Jourdain wrote on problems of cardinal multiplication, especially in *1908c* and *1908d*.

18. *May–July 1907 : individuals and the axiom of infinity*

Russell's paper on 'The existential import of propositions'[1] was part of his discussion with H. MacColl of a problem which is still alive today ; the legitimacy of many-valued logics. Many of their arguments were marred by conflations of implication, inference, and the conditional, by MacColl's identification of the empty class with the class of 'unreal' things and by Russell's various uses of the word 'existence'.[2] Although Russell had stressed in his paper the distinction between physical or abstract existence and the non-emptiness of classes, he did not fully clarify his other uses of the word, and in a letter of 31 May thanking him for the offprint Jourdain picked up this point :

> There has been a lot of confusion between logic and mathematics because 'existence' in mathematics has been held to mean *also* (besides your *b* [namely, the " existence " of a class]) being an entity. Thus mathematicians say : 'A complex number with more than two independent unities, and satisfying all the formal laws of arithmetic, does not *exist*' ; when they mean 'is not an entity', or 'the *class* of such complex numbers does not exist' in your sense *b* ($\sim\exists$). Mathematicians like Poincaré feel superior to symbolic logic because they think that mathematics considers the existence of individuals, while logic avowedly does not.[3] The whole confusion is verbal, but it seems to me that it ought to be pointed out. It led to my thinking that your criterion of 'existence' was insufficient, because it allowed 'the ordinal of ordinals' to exist.

In reply the next day Russell admitted :

> My paper on the Existential Import of Propositions ought to have mentioned the existence of described individuals. You know that my paper 'On Denoting' is summed up in the following definition (where '$(\imath x)(\phi x)$' means 'the x which satisfies ϕx') :
>
> $$\psi\{(\imath x)(\phi x)\} \,.\, = : (\exists c) : \phi x \,.\, \equiv_x \,.\, x = c : \psi c \quad \text{Df.}$$

[1] Russell *1905a*. This paper and some of MacColl's are gathered together in Russell *1973a*, 98–102 and 308–322.

[2] Jourdain commented on such points in his article on MacColl (*1912a*, 225–234). Russell's most substantial essay on non-classical logics is the manuscript 'Necessity and possibility' (about 1900–1901).

[3] Presumably Jourdain had in mind papers such as Poincaré's *1905a* and *1906a*. For commentary on such questions, see Mooij *1966a*, Bowne *1966a* and Chihara *1973a*.

Then I further define

$$\mathrm{E!}(\imath x)(\phi x) \,.\, = : (\exists c) : \phi x \,.\, \equiv_x \,.\, x = c \quad \text{Df.}$$

This E gives the ' existence ' of a described individual. It is the condition of there being any true propositions at all in which $(\imath x)(\phi x)$ appears grammatically as subject, or, more precisely, has a ' primary occurrence '.[1]

Existence was also the theme of the next section of Jourdain's letter of 31 May, although this time it was existence in an abstract rather than a conventional sense ; namely, Russell's recently published new proof of the axiom of infinity. He seems to have developed it as an off-shoot of his no-classes theories, and he presented it immediately after one of his summary accounts of them as follows :

> We assume as a primitive proposition that, given any proposition p, there is at least one entity u which is not explicitly mentioned in stating p. We also assume that what is true for all values of x is true for some value of x (or some equivalent of this assumption). This is equivalent to the assumption that there is at least one entity.
>
> Given one entity a, we have the proposition $a = a$; and by the axiom at the beginning of the last paragraph, there is an entity u such that u is not mentioned in ' $a = a$ '. This entity is not a, since a is mentioned in ' $a = a$ '. Hence there are at least two entities. There will be similarly an entity not mentioned in ' $a = u$ ', which must be neither a nor u. We can in this way show that, if n is any finite number, there are more than n entities, and by taking propositions into account, we can manufacture $\aleph_0$ entities. E.g. put
>
> $$p_0 \,.\, = \,.\, a = u, \quad p_{n+1} \,.\, = \,.\, p_n = u \,; \quad \text{[Dfs]}$$
>
> it is not hard to prove that the successive p's are all different, and that there are therefore at least $\aleph_0$ entities.[2]

In his letter of 31 May Jourdain tried to use this kind of proof to construct more than a denumerable infinity of objects :

> There is an entity u (Pp). Put :
>
> $$p_0 \,.\, = \,.\, u = u, \quad p_{n+1} \,.\, = \,.\, p_n = u \quad \text{Dfs} \,; \qquad [(18.1)]$$

[1] Compare Russell *1905b*, 489–491 (*1956a*, 52–54 ; *1973a*, 114–117), and pp. 70–71 above.

[2] Russell *1906c*, 638–639 (but quoted from the original English version as published in *1973a*, 202–203, with a footnote omitted).

thus we manufacture an ω of *different* p's ; but this way can't lead to *more* than an ω.

Now, with the same Pp,[1] let us put

$$p_0 . = . u \sim = . u,^{2} \quad p_1 . = . u \sim = p_0 \quad \text{Dfs} \qquad [(18.2)]$$

and, in general, when we have any well-ordered series $p_0, p_1, \ldots p_\zeta, \ldots$ of p's, we define

$$p_\gamma . = . `u = \text{none of the } p_\zeta\text{'s }'.^{3} \quad \text{Df} \qquad [(18.3)]$$

The suffixes attached to the p's are not, of course, to be thought of as *numbers*, but merely as convenient marks for distinguishing the p's.

The series of *all* these p's (if there is such a thing) can not give rise to a proposition *which is also a* p : 'a = none of the p's'. This proposition cannot be one of the p's (for if it were, it would be defined by itself).

Thus there is no p corresponding to Burali-Forti's 'type of all ordinals'. There is no such type, though I don't see why we shouldn't speak of 'the series of all ordinals' (unless the type of a series (of ordinals) is defined, as seems permissible), *as* this series ; thus, having defined the finite ordinals, we put

$$\omega = \text{the series } 1, 2, \ldots \quad \text{Df.}$$

Your solution of Burali-Forti fails except when the series of entities is limited in extent.[4] Thus, if all the entities we can manufacture form a series of ordinal ω_α, we can only prove the existence of $\omega_{\alpha+1}$ as an ordinal of ordinals, and we can't prove the existence of $\omega_{\alpha+\omega}$ if we don't admit a Cls^{ω}. If we can define a series of entities which is ordinally similar to 'the series of all ordinals', we don't get an ascent of logical type on going up the series of ordinals.

The only point, of course, is whether the definition

$$p_\gamma . = . `u = \text{none of the } p\text{'s already formed }'$$

[1] Jourdain added the footnote : 'I think we need (?) also the Pp : If p is a proposition or a class (to express it shortly) of propositions, $a \sim = p$ is a proposition (an entity)'. Presumably we should read 'u' for 'a', and interpret this assumption as asserting that u is an individual.

All ellipsis dots in this extract are Jourdain's own.

[2] Jourdain added the footnote : 'This proposition is always false : if we want a true proposition, use "$u \sim = u$" where u is assumed (Pp) to be another entity'.

[3] Jourdain added the footnote : 'This requires that no two p's are the same. If we put p_γ = "u = one of the p_ζ's", we would have p_γ = one of the preceding p's'.

[4] Presumably Jourdain had in mind Russell's rejection on p. 77 above of 'the whole notion of ordinals'.

can be expressed symbolically, and without assuming that ' the p's already formed ' form a class. Can it ?

Jourdain's final question raises only one of the problems associated both with his argument and with Russell's proof of the axiom of infinity. A well-formed version of his definition (18.3) of p_γ would involve quantification over the sufficial ordinals :

$$p_\gamma . = . (\zeta)\{\zeta < \gamma . \supset . u \sim = p_\zeta\} \quad \text{Df}, \qquad (18.4)$$

when it may not be the case that, in his phrase, ' the suffixes attached to the p's are . . . convenient marks for distinguishing the p's '.[1] In addition, it is not clear why the form of (18.3) could not be applied to Russell's sequence (18.1) of definitions as well as to Jourdain's alternative sequence (18.2), while in both cases (as Jourdain feared) classes would be assumed in defining by induction. Further, both types of definition rest on the relationship of identity (or non-identity) of a proposition p with an individual u, whereas we mentioned on page 76 above that the substitutional theory (of which both these arguments seem to be off-shoots) cannot operate successfully if identity of proposition with individual is allowed.

However, the era of the substitutional theory was now over, and Russell mentioned one of its features in his reply of 1 June to Jourdain—the acceptance of propositions as abstract objects (see page 92 above)—in reporting his change of mind about the provability of the axiom of infinity :

> Consideration of the paradox of the *liar* and its analogues has led me to be chary of treating propositions as entities. I therefore no longer regard as valid the proof of the existence of $\aleph_0$ entities by putting
>
> $$p_0 . = . u = u \quad [\text{Df}] \qquad p_{n+1} . = . p_n = u \quad [\text{Df}]$$
>
> nor consequently, the method you give for going further. I now think that existence-theorems beyond the finite require a definite assumption that the number of entities is not finite. I put
>
> $$\text{Infin ax} . = . \alpha \varepsilon \text{NC Induct} . \supset_\alpha . \exists ! \alpha(\text{Indiv}) \quad \text{Df}^2$$
>
> where
>
> $$\text{NC induct} = \hat{\alpha}\{0 \varepsilon \beta : y \varepsilon \beta . \supset_y . y + 1 \varepsilon \beta : \supset_\beta . \alpha \varepsilon \beta\} \quad \text{Df}$$

[1] Compare Poincaré's comment in *1909a*, art. 3 on assuming ordinals in defining types of transfinite order (which Russell eschewed, of course).

[2] Russell seems to present the axiom of infinity as a definition of ' Infin ax '. He first published the axiom in *1908a*, 258 (*1956a*, 97 ; van Heijenoort *1967a*, 179).

and α(Indiv) means ' an α composed of individuals ' (as opposed to classes, relations etc.).[1] All the existence-theorems that can reasonably be expected follow from *Infin ax* ; i.e. ω, η, $\aleph_0$, $2^{\aleph_0}$, $\aleph_\nu$ (for finite ν), etc. I do not assume *Infin ax* to be true, but state it explicitly as hypothetical wherever I use it. It goes with *Mult ax*, which together make up the two essentially arithmetical axioms.

He continued with a definition of the multiplicative class which is a repetition almost verbatim from his last letter (page 100 above). Then he rejected Jourdain's criticism of his solution of Burali-Forti's paradox when the series of constructible objects is restricted :

> I think there is an error when you say my solution of Burali-Forti fails except when the series of entities is limited. For Burali-Forti (or Cantor or cardinals) [sic] *proves* that *whatever* the NO of all entities may be, that of all entity-ordinals is greater. And according to the theory of types, such a phrase as ' the series of *all* ordinals ' is devoid of meaning, unless *all* is interpreted as meaning ' all of the nth type '.

However, there is still force in Jourdain's criticism, for Russell has only a finite number of types in his system and so ω_ω is indefinable in it.

For the first time in the correspondence Russell has associated the multiplicative axiom with the axiom of infinity. He described them here as ' essentially arithmetical ' axioms, which seems an unnecessary phrase for a logicist, for whom anything essentially arithmetical would certainly be logical also. But Russell was very unsure about the multiplicative axiom, for in his later writings he said that it was logicistically true (*1927a*, 299), that it refuted the definition of pure mathematics given in *The Principles* (*1931a*, 477), and that he was undecided on the matter (*1919a*, 124 ; *1937a*, viii, where he failed to mention Fraenkel's independence proof in *1922a*). However, he was not vacillatory about the axiom of infinity, as the following brief account (condensed from my *1974a*, 402–405) of a development in 1911 shows.

In his lecture of March 1911 to the Société Mathématique de France Russell at last made explicit one of the main reasons for his reluctance to assume the axiom of infinity : ' *individual* signifies *being of the actual world, as opposed to the beings of logic* '.[2] This view seems strange

[1] We have here an example of a notation specifying a particular type. Compare the definition of ' Infin ax (α) ' in *PM*, ✳120.04, the assignment of cardinals to types in ✳102, and the use of type suffices in general in ✳65.

[2] Russell *1911a*, 23 (p. 163 below) ; his italics. Note also *1919c*, 205 (*1956a*, 240) on the Wittgensteinian view that the number (or even existence) of individuals is not a logical question.

for a logicist, especially one who wrote elsewhere that ' in pure mathematics, as such, we do not consider actual objects existing in the actual world ' (*1904b*, 593), for it appears to mean that logic (and therefore mathematics also) are empirical sciences in that they require a physical world in which these individuals can exist. But it shows why individuals could not readily be assumed ; they belonged to the world of sticks and stones, not to the world of logic. He seems to have regarded such assumptions as antecedent to, *but not* part of, pure mathematics, which for him contained *only* conditional sentences of the form ' $p \rightarrow q$ ' (see, for example, *1903a*, arts. 1 and 412).

In contrast to the lecture, the first volume of *Principia Mathematica*, published late in 1910, was not helpful on the status of individuals. Phrases such as ' something which exists on its own account ' (p. 162) and ' neither a proposition nor a function ' (pp. 51, 132 ; see also pp. 93, 161), are unilluminating. In particular, they do not reconcile the apparent contradiction between the assertion that ' it is unnecessary, in practice, to know what objects belong to the lowest type ' (p. 161) and the characterisation of individuals as ' destitute of complexity ' (*1908a*, 238 (*1956a*, 76 ; van Heijenoort *1967a*, 167)).

But the most pertinent question left unanswered in this volume was the ***number*** of assumed individuals. Russell spoke of the universal class for each type (including that of individuals) and of the class ' Indiv ' of individuals (p. 347) ; but the axiom was not stated in the volume, and there are also a number of mentions that *only one* individual is assumed to exist.[1] And in fact, this was the situation ; the axiom of infinity was avoided, and only one (presumably physical) individual was assumed, in order to avoid the interpretation of quantifiers over an empty universe. (This assumption violated Russell's avoidance of definition under hypothesis discussed on pages 29–31 above.)

However, when preparing the section on cardinal arithmetic for the second volume of *Principia Mathematica*, Whitehead had assumed the axiom and so made existent cardinals which one individual on its own could not provide ; for with only one individual only 0 and 1 are definable for the type of individuals, 0, 1 and 2 for the type of classes, and so on. In January 1911 he stopped the printing of the volume and from then until May he reworked the whole theory. The most substantial modifications were a new ' Prefatory Statement of Symbolic Conventions ' and a heavily revised ✳126, which purported to explain how the restrictions of type were to operate.

[1] *PM*, vol. 1, 62, 208, 216, 225–226 and 335. Note also Russell *1919a*, 203, and *1911a*, 22–23 (p. 162 below). This point is often forgotten in commentaries on Russell's type theory. On the difficulties for the interpretation of names of individuals caused by individuals' logical structurelessness, see Prior *1965a* ; and on the logicists' ' curious mixture of empiricism and rationalism ', see Darbon *1949a*, 3–5.

The only surviving documents which deal with this strange affair are a few letters from Whitehead which luckily Russell preserved. In particular, there are no surviving letters to or from Jourdain for the first half of 1911 which might shed light on it. So we leave it now and return to their correspondence of the summer of 1907. We close this section with a reminder of a different kind from usual of how long ago their discussion took place. Jourdain's letter of 31 May had ended with an invitation for Russell to visit him at Broadwindsor. Russell agreed on 8 July to incorporate the visit on 21 July during a bicycling holiday, and he sent cards to report his progress :

Friday July 19/07
Shottermill[1]

I will reach you probably about 4 on Sunday, but as I am bicycling I can't be sure of the exact time. I am sending my bag to Crewkerne station, in hopes there is some way of getting it to you. It will reach the station some time tomorrow (Saturday).

B. Russell.

Southampton July 20/07
Saturday

I may succeed in reaching you by 1 o'clock, tomorrow, but I am likely to be later. I don't know whether my bag will have been sent up from Crewkerne ; if not, I suppose one could get a cart or something to go and fetch it tomorrow. I don't know where I shall sleep tonight—I shall ride as far as I can.

19. *March–October 1908 : Zermelo and his axiom system*

From this time on the correspondence tended to occur less frequently, although the quality of the exchanges remained high. Russell was now reporting progress on a programme of work whose outlines had been decided, rather than describing his latest uncertainties and changes of mind.

On 18 January Russell thanked Jourdain for ' sending me the proof, which I return '. It seems as if Jourdain sent Russell the galleys of his *1908b*, a complimentary note on Russell's *1907a* on ' The study of mathematics ', for Russell continued this letter with these remarks of dubious historical accuracy :

[1] Shottermill is very near to Haslemere, where Russell had been living when his correspondence with Jourdain started in 1902. Perhaps this was the bicycle trip during which Russell found difficulty in learning the way to Winchester (*1956b*, 157). No record survives of their discussions during this visit.

As to the Calculus and the Infinitesimal, I think the natural order of discovery is often different from the easiest order of learning, because an idea may be hard to think of in the first instance, but easy to apprehend when some one else suggests it. E.g. the Copernican system was hard to think of, but is easier to apprehend than the Ptolemaic. I dare say when people first took to the Copernican system they thought it so difficult that the young ought first to learn the other and then have Copernicus as a correction ; but this has proved an error.

Jourdain's next letter is lost, though there are some notes in the notebooks dated 8 March 1908 on the ' foundations of aggregates ' from which it may have been derived. For in his reply of 15 March Russell commented as follows on Zermelo's new paper *1908b*, which contained the first full-scale axiomatisation of set theory :

I have only read Zermelo's article once as yet, and not carefully, except his new proof of Schröder–Bernstein [prop. 27], which delighted me. I agree with your criticisms of him entirely. I thought his axiom for avoiding illegitimate classes so vague as to be useless ; also, since he does not recognize the theory of types, I suspect that his axioms will not really avoid contradictions, i.e. I suspect new contradictions could be manufactured specially designed to be consistent with his axioms. For I feel more and more certain that the solution lies in types as generated by the vicious circle principle, i.e. the principle ' No totality can contain members defined in terms of itself ', or ' anything which contains an apparent variable must not be a possible value of that apparent variable '.

But I think Zermelo a very able man, and on the whole I admire his work greatly. I am much obliged to you for writing to him about me and referring him to my articles.

I am going to Rome for the mathematical congress in April ; I wonder whether he will be there.

The tone of Russell's estimation of Zermelo reflects well the attitudes to foundational studies that could be taken at this time. Russell was delighted with Zermelo's new proof of the Schröder–Bernstein theorem (presumably for its avoidance of assumption of infinite selections or mathematical induction). However, he could not seriously consider Zermelo's approach, especially, one imagines, for its avoidance of the more general philosophical issues with which he personally was so concerned. When he wrote to Jourdain that ' the solution lies in types ', he was referring to his method not as a way of *avoiding* the paradoxes

but *the* way of *solving* them. Such an attitude was the only common feature of the various methods of solving paradoxes, and it was encouraged by the fact that each solution had drawbacks of some significant kind or other. For example, Russell pointed out to Jourdain an important lacuna in Zermelo's system ; that the axiom of separation (axiom III) required that a propositional function be ' definite for all members of a class M ' but lacked a precise criterion for definiteness. For followers of Zermelo this was to be one of their major problems.[1]

There is no trace of Jourdain's reply to this letter, but Zermelo must have remained in the discussions, for Russell wrote again on 30 March :

> The proof you report from Zermelo as to there being an infinite number of individuals is of a kind which I now consider fallacious, because it violates the principle of types. Zermelo sent me his papers, which I had read when they came out, as I take in the Mathematische Annalen. I sent him mine, with a polite letter.[2] I had only one copy of my London Mathematical Society paper [*1906a*] left, so I asked him to return it to me in Rome. I am sorry I have none to spare to send to you.

Russell's remark on the ' proof ' of an infinity of individuals is puzzling, since (like Russell now) Zermelo assumed an *axiom* of infinity in his system (*1908b*, axiom VII). In Russellian notation, it takes the form that there exists at least one class α such that

$$\Lambda\epsilon\alpha : (x) : x\epsilon\alpha \,.\, \supset \,.\, \iota'x\epsilon\alpha, \qquad (19.1)$$

and thus violates types, as Russell mentioned. Perhaps the report from Zermelo of the infinitude of individuals came to Jourdain in correspondence ; if so, it is now lost.[3] Russell continued in this letter to describe the progress of his own work : ' Whitehead and I are making good progress with *Principia Mathematica*. I find the doctrine of types

[1] Improvements in this axiom were first made by Weyl *1910a*, Fraenkel *1922a* and Skolem *1922a*. On the history of Zermelo's system see Cavaillès *1938a*, ch. 3, and Fraenkel *1958a*. For comments on Russell's logical ideas of this time vis-à-vis the development of set theory, see Weyl *1946a* and Moss *1972a*.

[2] This letter was written in German, and survives in Zermelo's *Nachlass*, which is kept in the Handschriftenabteilung of the Universitätsbibliothek at Freiburg im Breisgau, West Germany. There are two other letters from Russell (also in German), written in 1912 and discussing Zermelo's contribution (*1913a*) to the forthcoming International Congress of Mathematicians to be held that year in Cambridge. The Russell Archives possesses part of the other side of this latter exchange.

[3] Jourdain's notebooks contain only an acknowledgement from Zermelo dated March 1905 of the receipt of one of Jourdain's offprints, and a draft letter from Jourdain of April 1906. Zermelo's *Nachlass* (see the preceding footnote) has no correspondence with Jourdain.

admirable when one comes to cardinals and the difficulties connected with them '.

In May 1908 Russell was elected a Fellow of the Royal Society for his achievements in mathematical logic (see my *1975b*). On 2 July he acknowledged a lost letter of congratulations from Jourdain, and then reported the publication in the *American Journal of Mathematics* of his *1908a* on ' Mathematical logic as based on theory of types ' and his visit in April to the International Congress of Mathematicians in Rome :

> I have an article in this month's American Journal, of which I will send you an offprint when I get one. The American Journal is very slow—they kept the manuscript a year before printing it . . . I enjoyed seeing Zermelo and Bernstein and others at the Congress in Rome. The other day I met Veblen of Princeton whom I found most agreeable and interesting.

Russell also hoped to try to arrange another visit to Jourdain during a holiday in the West country,[1] but he apologised on 19 August from Braunton, Devon for not having had time to come over. Back home at Bagley Wood on 20 October he answered another lost letter from Jourdain, this time concerning Lady Welby, one of a number of ladies of the time who worked in logic and philosophy.[2] Her speciality was the foundations of language, which won for her the friendship of C. S. Peirce but markedly less enthusiasm from Russell :

> Many thanks for your amusing letter about Lady Welby. As she has not yet written to me, it seems you were successful in persuading her that it is not my business to know about the habits of rooks. I have in the past been very nearly rude to her, in refusing to go there, because I found it was quite impossible to be sincere if I saw her. I think it is very wrong of all these philosophers to encourage her as they do, and I don't want to be a party to it ; at the same time, when one is with her, one can't be as rude as truth requires.

Russell also alluded to his campaign the previous year for the by-election at Wimbledon in this letter :

[1] This is the passage omitted in the extract above. The letter began with reference to W. H. and G. C. Young : ' I am amused at the thought of your having the Youngs as neighbours. I should suppose that he might possibly become a little trying in the long run '. My *1972b* provides some information on the Youngs' personalities.

[2] Others include Mary Boole, Sophie Bryant, Constance Jones, Jourdain's sister Eleanor (whose *1911a* Jourdain helped to prepare for publication) and Christine Ladd-Franklin.

> William James writes to me to say ' My dying words to you are : " Say goodbye to mathematical logic if you wish to preserve your relations with concrete realities " '. I would much rather, of the two, preserve my relations with symbolic logic ; and if I wanted ' concrete realities ', I should certainly give up a student's life : you get more reality in a week of standing for Parliament than in a lifetime of study.[1]

20. *April 1909 : Russell's conception of logic, and a Wittgenstein curiosity*

There is no sign of correspondence during the winter of 1908–1909, but on 17 April 1909 Russell wrote from Clovelly, Devon to propose a visit to Broadwindsor. It took place three days later, and Jourdain wrote a lengthy report of their conversation in one of his notebooks.

Three passages are very striking. The opening paragraph provides the first :

> On April 20 1909, Russell came and had lunch and tea. In talk, he said, in correction of my (badly expressed) idea that none of the principles of logic can be denied without assuming one or more of their principles, that though a principle can be denied with impunity, ***no contradiction can be drawn from this denial.*** Thus, we cannot conclude, from the proposition that the law of excluded middle is not true, that it is false, without using it.

Perhaps unintentionally, this remark of Russell goes close to the heart of his conception of logic, in a way which may be described as follows. The principal idea that Russell learnt from Peano was that there was a possibility of expressing the *whole* of (pure) mathematics in a unified symbolism. In this respect his logicism differed fundamentally from Frege's, which was confined to arithmetic and explicitly excluded geometry (see Frege's *1885a* and *1906a*). Peano abstained from philosophising about logic (see Geymonat *1955a*, 56–62) ; indeed, he was careful to distinguish between logical and mathematical notions in his writings. But Russell conceived from Peano's work his logicist philosophy of mathematics, including the Peanesque practice of incorporating the meta-theory of the system in itself. Thus he generated many conflations of use and mention, of which we have seen examples in this correspondence. From this point of view he made little advance on Peano. For example, Peano had no rules of inference at all : Russell

[1] On this campaign see Russell *1967a*, 153–155. James's letter, dated 4 October 1908, was published in Russell *1967a*, 197–198.

introduced ' principles of inference ', but associated them with implication as well as with inference as we now understand them, for they were among his ' primitive propositions ' (his Peanesque term, to cover axioms as well as rules of inference).[1] Another point of contrast with Frege arises here, for Frege was usually careful about use-mention distinctions (though even he implicitly assumed rules of substitution). Had Russell been converted to mathematical logic by Frege rather than Peano, he might well have constructed a much more careful philosophical system while still retaining the benefits that Peanism provided.

Now because Russell's Peanesque logic embraces its meta-theory, it is *strictly impossible* to talk *about* it, for such talk would go illegitimately outside the bounds of logical discourse. For example, as Jourdain recorded, if we deny that the law of excluded middle is true we cannot then deduce that it is false, for such a deduction would (violate the vicious circle principle and) assume the truth denied.[2] In modern terminology, there is no meta-theory in which that truth can be sited.

The first point from Jourdain's notes highlighted differences between the conception of logic held by Peano and Russell and that due to Frege. But there were many similarities too, such as the use of propositional functions and the distinction between an object and its unit class, and they appeared in the second passage that we quote from Jourdain's notes :

> A propos of my suggested division of the history of symbolic logic and ideography (Russell likes the term ' mathematical logic ',—a necessary preliminary to logical mathematics[3]) into : (1) Use of variables (goes back to Aristotle) ; (2) Use of propositional functions (MacColl seems to have been the first to do so ; but he was not quite clear about them—in expression, at least[4]) ; (3) Introduc-

[1] See Russell *1903a*, chs. 2–3, esp. art. 18 ; and compare *PM*, ✳1–✳3 and ✳9 *passim*. Among the various studies of Russell's conflation of implication with inference and of related problems in his ' theory of deduction ', see B. Bernstein *1931a*, *1932a* and *1933a*, Nelson *1934a*, Lee *1958a*, Vuillemin *1968a* (esp. pp. 12–37), Jager *1972a*, 117–137, and Feibleman *1973a*. Peano's letters to Russell are published in Kennedy *1975a*.

[2] Compare Russell's remarks on the law of excluded middle in *1906c*, 644–645 (*1973a*, 208–209), and *PM*, vol. 1, 129. On his conception of logic, see his manuscripts published in *1973a*, 273–283 and 295–306, and also the unpublished ' What is logic ? ' (October 1912). For some later views on logicism, see Quine *1970a* and Bostock *1974a*.

[3] According to Hall *1972a*, 166, de Morgan introduced the term ' mathematical logic ' in *1858a*, 176 (*1966a*, 78). He was already using it in 1857 in correspondence with Hamilton (see Graves *1889a*, 521, and compare pp. 541–542, 550 and 552).

[4] In his *1877a*, 181, MacColl interpreted the Universal Affirmative ' All X is Y ' as : ' if an individual belongs to the class X it belongs also to Y '. There are seeds here of a theory of the propositional function (and of quantification over individuals) though they were not developed. Russell acknowledged this element of priority to MacColl in *1906d*, 255, correcting an attribution in *1905a*, 400 (*1973a*, 100) of this interpretation of the Universal Affirmative to Peano. Jourdain also noted MacColl's priority in *1910a*, 99 ; see also *1912a*, 224–225.

tion of ***relations*** (compare especially Frege on the development of symbolic logic with that of mathematics[1]) ; Russell remarked that he would not include (3) as one of the main stages ; the algebra of relations seems to him above all a *technical* discipline.

At first sight Russell's judgement seems to be very modest, coming from the logician who augmented Peano's system with a logic of relations and used relations so brilliantly thereafter. But his reference to the ' algebra of relations ' suggests that his principal aim was to play down the significance of the Boolean approach to relations as developed by Peirce and especially Schröder in his *1895a*. In 1913 he was to expand greatly on this theme when he read the thesis by the young N. Wiener on the Schröderian logic of relations as compared with that of *Principia Mathematica*. Wiener established a considerable measure of structural similarity between the two systems ; but he failed to see the full significance of the distinctions which are prominent in the Russell–Peano–Frege approach but absent from Schröder's system (between membership and inclusion, an object and its unit class, and so on), and Russell stressed such points to him very forcibly. For example, he told Wiener that the initial source of inspiration for him towards Peanoism was Peano's use of a notation for ' the ', a factor of significance in the development of distinctions of which Schröder had been unaware.[2]

The third passage from Jourdain's notes is very unexpected. The main bulk of the notes dealt with the definability of classes in view of the paradoxes and with matters concerning axioms of choice, and covered ground which we have explored already. But they begin as follows :

> Russell said that the views I gave in a reply to Wittgenstein (who had ' solved ' Russell's contradiction) agree with his own. These views are : The difficulty seems to me to be as follows. In certain cases (e.g. Burali-Forti's case, Russell's ' class ' $\dot{x}(x \sim \varepsilon x)$, Epimenides' remark) we get what seem to be meaningless *limiting cases* of statements which are not meaningless.

The opinion of Wittgenstein is not in itself remarkable. The importance of the passage lies in its *date*—that Wittgenstein should have been interested in Russell's paradox already in 1909, two and a

[1] Possibly an allusion to the preface of Frege's *1879a*, though various parts of his writings are relevant.

[2] Wiener's thesis *1913a* remains unpublished. For a description of it and of Wiener's discussion of its contents with Russell see my *1975a*. ' the ' is dealt with in sect. 4.2, and shows that art. 63 of Russell's *1903a* is of great historical significance. Russell's criticisms of Schröder are quoted in sect. 7 ; six pages of his notes on Schröder survive in the Russell Archives.

half years before the normal attribution of his contact with Russell.[1]

Various points need to be clarified. In the first place, our first quotation from these notes on page 112 above gave the date explicitly as 20 April 1909. Further, Jourdain's handwriting (which tended to vary with the current state of his paralysis) is clearer here than usual and undoubtedly says 'Wittgenstein' (in full) at this point. The notes appear to have been written at one sitting, and are preceded in the notebook by Russell's letter of 17 April arranging the visit and followed by a draft of a letter to Russell of 28 April which we have yet to consider. Both notebooks give throughout the impression of working documents, and rather chaotically organised documents at that. It seems very unlikely that he compiled them years later as a retrospective catalogue of previous correspondence; the physical effort involved would have been daunting even for him, and the organisation would surely have been better. Even if he did, the nature of the reference to Wittgenstein remains unexplained. Finally, there is no evidence that anyone else called 'Wittgenstein' was trying then to 'solve' Russell's paradox.

Thus the legitimacy of the passage seems indisputable; but the details of the relationship between Wittgenstein, Jourdain and Russell are not clear. Jourdain's reference to a 'reply' to Wittgenstein indicates that the contact was with him rather than with Russell, and probably in a written rather than a verbal form. Wittgenstein was at Manchester University or abroad at this time (see von Wright *1966a*, 5), and may have seen Jourdain's address at the bottom of one of his papers. If he did write, then unfortunately Jourdain did not put the correspondence in his surviving notebooks. No direct contact with Russell is implied, but the subject matter is Russell's paradox, and it seems impossible that Jourdain would not have mentioned to Russell *by name* this new correspondent who had views on 'solving' his paradox.

So we must leave this historical problem unsolved. The evidence of Wittgenstein's acquaintance with Russell's work seems firm, but it is not reconciled with other information on their relationship.[2] Perhaps some other documents will turn up one day to clarify the situation.

In a letter of 28 April Jourdain took up a claim by Russell in their conversation that the multiplicative axiom was needed in the proof that the limit ordinal of a denumerable series of ordinals of the second

[1] See, for example, von Wright *1966a*, 5. Russell is very vague on the date of his acquaintance with Wittgenstein and his work: see *1951a*, 298, *1959a*, 112, and *1968a*, 98.

[2] In his correspondence with Ottoline Morrell (kept at the Humanities Research Center, University of Texas at Austin, U.S.A., and drawn on here by courtesy of notes made by K. Blackwell and now kept in the Russell Archives) Russell mentioned taking Wittgenstein to 'meet' Jourdain on 19 February 1913. The impression is given of a first meeting. B. F. McGuinness, who is preparing a biography of Wittgenstein, tells me that he has found no evidence either corroborating or refuting Jourdain's note.

number-class also belong to that number-class. To Jourdain the axiom was unnecessary, since each sub-class was well-ordered ; but when Russell replied two days later he was careful to point out where the axiom was needed :

> As regards the proposition that the limit of an ω of numbers of the second class is of the second class, you have not realized where the multiplicative axiom comes in. It comes in in picking out one among possible correlations of two given $\aleph_0$'s.[1] Thus: suppose we define the second number-class as those numbers which have $\aleph_0$ predecessors. Let $\alpha_1, \alpha_2, \ldots \alpha_\nu, \ldots$ be an ω of such numbers. Then between α_ν and $\alpha_{\nu+1}$ there are at most $\aleph_0$ numbers. Assume that there are $\aleph_0$ numbers, for every ν. The series has a limit α_ω, and we have to prove that α_ω is of the second number-class. For this purpose, we have to prove that the number of its predecessors is $\aleph_0$. Now the number is formed of $\aleph_0$ mutually exclusive $\aleph_0$'s, namely the predecessors of α_1, and the numbers between α_ν and $\alpha_{\nu+1}$ for every ν (including α_ν and excluding $\alpha_{\nu+1}$). If we can pick out, for each of these $\aleph_0$'s, *one* correlation with a given $\aleph_0$, say the finite numbers, we can prove that their sum is an $\aleph_0$; if not, not. Thus what is needed is to be able to give *one* rule, for each series whose number is of the second class, by which it can be transformed into an ω. Such a rule may be definable by induction (in the generalized sense), but in the absence of such a rule the theorem remains unproved. It is not to the classes of series, but to the classes of their correlations with a given $\aleph_0$, that the multiplicative axiom has to be applied.[2] The problem is intimately connected with Hardy's problem : to pick out one ω for each limit-number of the second class, such that the ω has the given number for its limit.[3] Much work stands or falls according as the proposition is true or false—e.g. most of Hausdorff's work.[4]

We began this section with Russell's conception of logic as evidenced by his remarks on law of excluded middle. We return to it for the next point. Possibly in reference to those remarks, Jourdain asked Russell in his letter of 28 April :

[1] Russell again used '$\aleph_0$' in this extract to refer both to the smallest transfinite cardinal and to a denumerable class.

[2] This theorem is in *PM*, ✳265.49. On the need of the multiplicative axiom for such results compare vol. 3, 170–171, and also Russell *1911a*, 30 (p. 170 below).

[3] An allusion to Hardy's construction of a class of cardinality $\aleph_1$ in *1903a*, 88–94 (see p. 47 above).

[4] Presumably an allusion to Hausdorff's *1906a*, *1907a*, *1907b* and *1908a*.

> When you enumerate the primitive propositions of *logic*, do you prove their independence by the usual method of giving certain interpretations to the primitive ideas, so that all but one (in turn) of the primitive propositions is verified ?

By the ' recognised methods ' Jourdain probably had in mind the intuitively meta-theoretic or model-theoretic investigations of the foundations of geometry, and of algebraic structures and Boolean algebras, which had been current for some decades.[1] Russell's conception of logic precluded such procedures, and his answer to Jourdain echoed the tone of his former remarks :

> I do not prove the independence of primitive propositions in logic by the recognised methods ; this is impossible as regards principles of inference, because you can't tell what follows from supposing them false : if they are true, they must be used in deducing consequences from the hypothesis that they are false, and altogether they are too fundamental to be treated by the recognized methods.

Unfortunately Russell did not continue and indicate how he would investigate the independence of his primitive propositions, or even if such a study were possible. Perhaps it is not surprising that *Principia Mathematica* was to turn out not to be an independent system.[2]

Jourdain raised another point about the fundamental principles of logic in his letter, this time concerning the following passage in F. H. Bradley's *The Principles of Logic* of 1883 on ' the principle of identity ' (*1883a*, 133) :

> [The principle] runs thus : ' Truth is at all times true ' . . . So stated the principle is not very clear, but perhaps it will find acceptance with most readers. What it means, however, is much more definite, and will be much less welcome. The real axiom of Identity is this : *What is true in one context is true in another.* Or, if any truth is stated so that a change of events will make it false, then it is not a genuine truth at all.
>
> To most readers this axiom, I have little doubt, will seem a false statement.

[1] Hilbert *1899a* was a prominent recent example of the investigation of independence described by Jourdain. For a history of axiomatic studies during this period see Cavaillès *1937a*.

[2] Among early studies see Daniell *1919a*, Bernays *1926a* and McKinsey *1935a*. The problem involves Russell's conflation of theory and meta-theory mentioned on p. 112 above. On the related question of consistency, which Russell also could not study properly, see especially Fitch *1974a*.

Bradley's double use of ' most readers ' is rather unfortunate, as Jourdain commented to Russell : ' It results that Bradley thought that at least one of his readers will accept a principle whose meaning seems to the reader false'. Russell did not quite agree : ' I looked up the page of Bradley, and was much amused. But the inference you draw does not strictly follow ; for Bradley gives 2 statements of his principle, and expects his readers to think that they express *different* principles'.

Jourdain devoted much of this letter to his plans for future work, and asked Russell's advice on the best tactics to secure as approbation for it a Fellowship of the Royal Society, which Russell had received in the previous year. Russell put Jourdain in his place on that point : ' I am not in a position to say what qualifications the Royal Society expects, being myself so very recently elected. I understand that it is etiquette that a man should not himself take any steps in the matter : apparently it depends upon the chance of some big-wig taking one up'.

21. *August 1909–January 1910 : convergence and continuity in* Principia Mathematica

At this time Jourdain was preparing a lengthy series of studies of significant figures in the history of mathematical logic. The articles that were eventually published dealt with Leibniz, Boole, MacColl, Frege, Peano and Jevons. Jourdain sent his manuscripts to those logicians still living, and incorporated their comments in footnotes. He also sent the manuscripts to Russell, and from August 1909 onwards they were mentioned in the correspondence from time to time. On the whole Russell's comments were of a general rather than a detailed character, but when the manuscripts were published in the *Quarterly Journal of Pure and Applied Mathematics* for 1910, 1912 and 1913[1] Jourdain acknowledged Russell's help at the beginning of each instalment. The first manuscript completed seems to have been the one on MacColl, which Jourdain sent to Russell on 28 August 1909. Russell replied on 5 September :

[1] Jourdain *1910d*, *1912a* and *1913a*. He intended also to write on Peirce, and in Peirce's *Nachlass* in Houghton Library, Harvard University, Cambridge, Mass., U.S.A., there is a draft letter of 5 December 1908 (undoubtedly addressed to Jourdain) in file L230a, answering a string of biographical questions. Among other points, Peirce stressed his recognition of the distinction between an object and its unit class (a point not properly recognised in his earlier published work on logic) and mentioned that his *1870a* on the logic of relatives ' was nearly complete before I had much acquaintance with de Morgan's paper ' *1860a* on the same subject. Part of this letter (then unidentified as to correspondent) is printed in Eisele-Halpern *1963a*, 121–122, and the whole letter will appear in Miss Eisele's forthcoming edition of Peirce's mathematical papers.

I have read you on MacColl with complete agreement, and found no criticism to make, except, perhaps, that you give scarcely enough exposition of his system before criticizing it. It seemed to me that if one had never read him, one might feel that one hardly had material enough, in your account, to follow your discussion adequately. I have not verified all your references, as many of the things you refer to are not in my library. I am very glad to find you so much in agreement with me as regards the points about which MacColl and I have differed.[1]

Jourdain's covering letter is lost, but it must have commented on possible anticipations of Frege's distinction between sense and reference, for Russell continued :

It would seem, from what you say in your letter, that Miss Jones's distinction of signification and denotation must be much the same as Frege's Sinn and Bedeutung. But of course *some* such distinction is a commonplace of logic, and everything turns on the form given to the distinction. I have neither Keynes nor Miss Jones here, or I would have looked up the point.[2]

The bulk of the manuscript of *Principia Mathematica* was taken to the Cambridge University Press in October 1909 (see my *1975c*), and now the letters often described completed sections. In particular, when Jourdain asked on 27 December about a general definition of a quotient which could be used for defining a differential quotient as well as for arithmetical division, Russell wrote on 25 January 1910 this magnificent account of the treatment in *Principia Mathematica* of arithmetic operations and the continuity of functions :

I have never found any definition of a quotient such as you want. But I think it not unlikely that something analogous to a differential could be defined generally. The definitions of + and × between cardinal integers are got as follows :

$x\downarrow y$ or $x\downarrow' y$ or $\downarrow' y' x$ denotes the ordered couple ' x to y ' i.e. the relation holding only between x and y, i.e.

$$[*55.01] \quad \hat{u}\hat{v}(u = x \,.\, v = y).^{3}$$

Λ_α denotes the null-class of the type to which α belongs. Then

[1] The article on MacColl is Jourdain *1912a*, 219–236 ; MacColl commented briefly on the manuscript before he died in 1909.

[2] The sources mentioned are Jones *1890a* and Keynes *1906a* ; Jourdain discussed the point in detail in *1912a*, 263.

[3] ' $\downarrow z$ ' denotes the relation of $v\downarrow z$ to z, as given in *PM*, ✳38.02 :

$$\downarrow z = \hat{u}\hat{v}(u = v\downarrow z) \quad \text{Df.}$$

$\downarrow\Lambda_\beta{}''\iota''\alpha$ is a class of α couples, of which a typical member is $(\iota'x)\downarrow\Lambda_\beta$, where $x\varepsilon\alpha$. Each of these couples is of the same type as $\alpha\downarrow\beta$. Similarly $\Lambda_\alpha\downarrow''\iota''\beta$ is a class of β couples, each of the same type as $\alpha\downarrow\beta$. Moreover the two classes of couples have no members in common. Hence without assuming that α and β are mutually exclusive, or that they are of the same type, we get a class, $\downarrow\Lambda_\beta{}''\iota''\alpha\cup\Lambda_\alpha\downarrow''\iota''\beta$, the number of whose terms is the sum of the members of α and β. Now write $\mathrm{N}_0\mathrm{c}'\alpha$ for the class of classes which are similar to α and of the same type as α. Then the sum of μ and ν is defined thus :

$$[110.02]\quad \mu+_c\nu=\hat{\xi}\{(\exists\alpha,\beta)\,.\,\mu=\mathrm{N}_0\mathrm{c}'\alpha\,.\,\nu=\mathrm{N}_0\mathrm{c}'\beta\,.\,\xi\ \mathrm{sm}\ (\downarrow\Lambda_\beta{}''\iota''\beta\cup\Lambda_\alpha\downarrow''\iota''\beta)\}\quad \mathrm{Df},$$

where ' sm ' means ' similar '.[1] The above definition is designed to obviate all difficulties as to types and existence. If μ and ν are not both Cardinals,

$$\mu+_c\nu=\Lambda.^{2}$$

For multiplication, we use the class of couples formed of a member of α and a member of β. Thus

$$[*113.03]\quad \mu\times_c\nu=\hat{\xi}\{(\exists\alpha,\beta)\,.\,\mu=\mathrm{N}_0\mathrm{c}'\alpha\,.\,\nu=\mathrm{N}_0\mathrm{c}'\beta\,.\,\xi\ \mathrm{sm}\ \hat{R}((\exists x,y)\,.\,x\varepsilon\alpha\,.\,y\varepsilon\beta\,.\,R=y\downarrow x)\}\quad \mathrm{Df}.$$

Here again,

$$\sim(\mu,\ \nu\varepsilon\mathrm{NC})\ .\ \supset\ .\ \mu\times_c\nu=\Lambda.$$

Thus these definitions do not serve to give a useful definition of μ/ν when $\sim(\mu,\ \nu\varepsilon\mathrm{NC})$.

In our ' Principia Mathematica ', which is now gone to the Press, we have a section on continuity of functions etc., without the use of numbers [*234]. I forget whether you know our definition (adapted from Frege) of the *ancestral relation* R_*, which is used incidentally in dealing with continuity. We put

$$[*90.01]\quad R_*=\hat{x}\hat{y}\{x\varepsilon\mathrm{C}'R:\breve{R}''\mu\subset\mu\ .\ x\varepsilon\mu\ .\ \supset_\mu\ .\ y\varepsilon\mu\}\quad \mathrm{Df}.^{3}$$

$$[*91.03]\quad \mathrm{Pot}'R=(\vec{\lceil}R)_*{}'R\quad \mathrm{Df},$$

[1] ' $\mathrm{N}_0\mathrm{c}'\alpha$ ' is called the ' homogeneous cardinal ' of α in *PM*, *103.01. It is important to note that $\mathrm{Nc}'\alpha$ is defined in terms of the similarity of α with classes from *every* type. On this definition of cardinal addition compare p. 37 above.

[2] This is an example of the avoidance of definition under hypothesis discussed on pp. 29–31 above ; if the hypothesis is not satisfied then the empty class is defined.

[3] Russell had used the ancestral relation in December 1904 when constructing a denumerable class on pp. 64–65 above. Note that x is now restricted to $\mathrm{C}'R$, as opposed to the 1904 definition ; and that ' μ ' now refers to a class, not necessarily a cardinal as earlier in this extract. According to Carnap *1963a*, 5–6, around 1913 Frege had worked this kind of mathematics in his own system.

where

[*38.02] $$|R = \hat{P}\hat{Q}(P = Q|R) \quad \text{Df}$$

[*91.05] $$R_{\text{po}} = \hat{s}\text{`Pot`}R \quad \text{Df.}$$

If R is transitive,

$$R_{\text{po}} = R \quad \text{and} \quad R_* = R \cup I \restriction \text{C`}R$$

(where I = identity).[1] In all cases, R_{po} is transitive [*91.56] and

[*91.54] $$R_* = R_{\text{po}} \cup I \restriction \text{C`}R.$$

To deal with convergency [sic] and continuity we proceed as follows. Let R be a function (i.e. a one-many relation), and let some or all of its arguments (i.e. of its converse domain) belong to a series Q, while some or all of its values (i.e. of its domain) belong to a series P.[2]

Suppose as the arguments become later in Q, the values 'ultimately' belong to a class α. If y is a member of the Q-series such that for all later arguments (y included) the value is a member of α, we have

$$R\text{``}\breve{Q}_*\text{`}y \subset \alpha.$$

Hence put

[*230.01] $$R\breve{Q}_{\text{cn}}\alpha = \text{C`}Q \cap \text{D`}R \cap \hat{y}(R\text{``}\breve{Q}_*\text{`}y \subset \alpha) \quad \text{Df}$$

where 'cn' stands for 'convergent', and put

[*230.02] $$Q_{\text{cn}} = \hat{R}\hat{\alpha}(\exists ! R\breve{Q}_{\text{cn}}\alpha) \quad \text{Df.}$$

Thus '$R\breve{Q}_{\text{cn}}\alpha$' means 'As the argument y to R increases in Q, the value $R\text{`}y$ ultimately becomes and remains a member of α'.[3] Put next

$$P\breve{R}_{\text{sc}}Q = p\text{`}P_*\text{``}\breve{Q}_{\text{cn}}\text{`}R \quad \text{Df.}[4]$$

Then '$y \varepsilon P\breve{R}_{\text{sc}}Q$' means that, as the argument increases in Q, the value ultimately becomes not less than y. (I speak of a 'less' value, meaning simply one which comes earlier in P.) Put

[*231.02] $$P\breve{R}_{\text{os}}Q = P\breve{R}_{\text{sc}}Q \cap \breve{P}\breve{R}_{\text{sc}}Q \quad \text{Df.}$$

[1] Russell forgot to include here *PM*, *35.02:

$$S \restriction \beta = \hat{x}\hat{y}(xSy \,.\, y \varepsilon \beta) \quad \text{Df.}$$

[2] Hence R maps from values to arguments, not vice versa.

[3] The sentence in quotation marks (or rather, a less loosely worded version of it) should have '$y \varepsilon R\breve{Q}_{\text{cn}}\alpha$' as its symbolic analogue.

[4] Add '$\cap \text{C`}P$' to this defining expression (as in *PM*, *231.01), since naturally the members of $P\breve{R}_{\text{sc}}Q$ must belong to the field of P.

Then '$P\breve{R}_{os}Q$' represents the 'ultimate oscillation' of the function; i.e. however far up the argument-series you go, you still find values belonging to the class $P\breve{R}_{os}Q$.

The above gives what we may regard as the ultimate oscillation for the argument ∞.[1] To obtain the ultimate oscillation as the argument approaches the limit of any class α,[2] we only have to replace Q by $Q\restriction\alpha$, where

[*36.01] $\qquad Q\restriction\alpha = \hat{x}\hat{y}(x, y\epsilon\alpha \,.\, xQy)$ Df.

For small technical reasons, $Q_*\restriction\alpha$ is more convenient than $Q\restriction\alpha$.[3] Thus we put

[*232.01] $\qquad (P\breve{R}Q)_{sc}{}'\alpha = P\breve{R}_{sc}(Q_*\restriction\alpha)$ Df

[*232.02] $\qquad (P\breve{R}Q)_{os}{}'\alpha = P\breve{R}_{os}(Q_*\restriction\alpha)$ Df.

Thus $(P\breve{R}Q)_{os}{}'\alpha$ is the ultimate oscillation as the argument approaches the limit of α. The ultimate oscillation as the argument approaches y is got by putting $\alpha = \vec{Q}'y$; but the above form is more general, since it works when α has no limit, i.e. defines an 'irrational' cut.

The four limits of a function for a given argument then result at once. I write '$\mathrm{limax}_p{}'\beta$' for the 'limit or maximum' of β in the series P. Put then

[*233.01] $\qquad (P\breve{R}Q)_{lmx}{}'\alpha = \mathrm{limax}_p{}'(P\breve{R}Q)_{sc}{}'\alpha$ Df

[*233.02] $\qquad R(PQ)'a = (P\breve{R}Q)_{lmx}{}'\breve{Q}'a$ Df.

Then the four limits of the function for the argument a are

$$R(PQ)'a, \quad R(\breve{P}Q)'a, \quad R(P\breve{Q})'a, \quad R(\breve{P}\breve{Q})'a.$$

If the function is continuous, these limits are all equal.

The continuity of functions is thus defined : Put

[*234.01] $\qquad \mathrm{sc}\,(P, Q)'R = \mathrm{C}'P \cap \hat{x}(\vec{P}_{po}{}''\breve{P}_{po}{}'x \subset \breve{Q}_{cn}{}'R)$ Df

[*234.02] $\qquad \mathrm{os}\,(P, Q)'R = \mathrm{sc}\,(P, Q)'R \cap \mathrm{sc}\,(\breve{P}, Q)'R$ Df

[*234.03] $\qquad \mathrm{ct}\,(P, Q)'R = \hat{a}\{R'a\epsilon\mathrm{os}\,(P, Q_*\restriction Q_{po}{}'a)'R - \mathrm{C}'P_1\}$ Df

[1] This phrase is used because $\mathrm{C}'Q$ (or $\mathrm{C}'R$) is sometimes the class of finite integers (and $\mathrm{D}'R$ is a sequence of real numbers); see *PM*, vol. 2, 687.

[2] More precisely, as the argument approaches x via members of any class α which has x as a limit-point.

[3] For if α is a unit class, then $Q\restriction\alpha$ will allow convergence to x only through $\vec{Q}'x$ (see *PM*, vol. 2, 708).

where P_1 means 'immediately preceding', i.e. $P \div P^2$. Thus '$-\mathrm{C}'P_1$' is the class of terms having no immediate predecessors or successors.[1]

[∗234.04] $\mathrm{contin}\,(P, Q)'R$

$$= \mathrm{ct}\,(P, Q)'R \cap \mathrm{ct}\,(P, \breve{Q})'R \quad \text{Df}$$

[∗234.05] $P\ \overline{\mathrm{contin}}\ Q$

$$= \hat{R}\{\exists!\mathrm{C}'Q \cap \mathrm{D}'R \,.\, \mathrm{C}'Q \cap \mathrm{D}'R \subset \mathrm{contin}\,(P, Q)'R\} \quad \text{Df.}$$

Then $\mathrm{contin}\,(P, Q)'R$ is the class of arguments for which R is continuous, and $P\ \overline{\mathrm{contin}}\ Q$ is the class of continuous functions.

We find that, if P is a compact series and Q is a series and $R''\vec{Q}'a \subset \mathrm{C}'P$, then

[∗234.522] $a \varepsilon \mathrm{ct}\,(P, Q)'R \,.\equiv.\, R(P, Q)'a = R(\breve{P}, Q)'a = R'a,$

i.e. the two limits of the function for approaches to a from below are equal to the value for the argument a. Hence with a similar hypothesis,

[∗234.6] $a \varepsilon \mathrm{contin}\,(P, Q)'R \,.\equiv.\, R(P, Q)'a = R(\breve{P}, Q)'a$

$$= R(P, Q)'a = R(\breve{P}, \breve{Q})'a = R'a.$$

This relates our definition to the usual definitions.

The definition by ε's and intervals is easily reduced to a form not containing numbers, and is then shown to be equivalent to ours. We put

$$P(z \vdash\dashv w),\ P(z \vdash w),\ P(z \dashv w),\ P(z - w)$$

for the interval from z to w in the series P, with or without its ends; i.e.

[∗121.013] $P(z \vdash\dashv w) = \overleftarrow{P}_*'z \cap \vec{P}_*'w$ Df

[∗121.012] $P(z \vdash w) = \overleftarrow{P}_*'z \cap \vec{P}_{\mathrm{po}}'w$ Df

[∗121.011] $P(z \dashv w) = \overleftarrow{P}_{\mathrm{po}}'z \cap \vec{P}_*'w$ Df

[∗121.01] $P(z - w) = \overleftarrow{P}_{\mathrm{po}}'z \cap \vec{P}_{\mathrm{po}}'w$ Df.

[1] 'P_1' is used in *PM*, ∗234.03 without this description or any cross-reference. In fact it is given by putting $\nu = 1$ in ∗121.02 (with ∗121.013):

$$P_\nu = \hat{x}\hat{y}\{\mathrm{N}_0\mathrm{c}'(\overleftarrow{P}_*'x \cap \vec{P}_*'y) = \nu + \mathrm{c}1\} \quad \text{Df.}$$

Thus xP_1y states that y is the immediate predecessor of x under P. The property described by Russell here is proved in ∗201.63.

By contrast with ∗234, I have preserved Russell's commas in '$\mathrm{ct}\,(P, Q)$' and '$\mathrm{contin}\,(P, Q)$' in this extract.

Then if P and Q are series and $R`a$ is a term in the middle of the P-series,

$$[*234.64] \quad a\varepsilon\text{contin}\,(P, Q)`R \;.\; \equiv \;:\; R`a \sim \varepsilon \breve{C}`P_1 \;:\; R`a\varepsilon P(z-w).$$
$$\supset_{z,\,w} . (\exists y, y') . a\varepsilon Q(y-y') . y, y'\varepsilon \breve{C}`R .$$
$$R``Q(y \mapsto y') \subset P(z-w).$$

The right hand side here gives the usual ε-definition of a point of continuity. This is the gist of what I can tell you on the subject.

As if that were not enough, Russell prefaced this analysis with an answer to two questions from Jourdain about the phenomenalism of E. Mach. Jourdain had asked :

> (a) Is it correct to say (as Mach and Boltzmann do) that, in mechanics and physics, our thoughts are models (mathematical constructions) of reality. And do you think that what philosophers mean when they talk of a ' postulate of the intelligibility of nature ' is the postulate that such a model is possible ?
>
> (b) Can one hold that the same circumstances never exactly recur in nature, but *do* in our model (which represents reality approximately, but is more handy than nature) ? Thus the law of uniformity does not apply to *nature* but does to our *model* ; thus Mach is right in saying that the world is not twice given, and you are speaking of the model when you say it is given as often as we please.

Jourdain's questions were connected with his interest in the foundations of mechanics (see especially his *1908e*), and seemed to be the continuation of a discussion of such matters from some previous occasion of which there is now no trace. We put on record Russell's only comments on the philosophy of science in this correspondence, noting especially the beginnings of his efforts to apply mathematical logic to new areas :

> (a) I think it is correct to say that in Physics our thoughts are models. But I don't think this is what philosophers mean by the ' postulate of the intelligibility of nature '. I think they mean something much more general and vague.[1]
>
> (b) One *can* hold that the same circumstances never recur in nature, but do in our model. But we have no reason to *know* that the circumstances do not recur in nature : we can only say it is not probable that they do. Your view of the ' uniformity of nature '

[1] Jourdain quoted this phrase in *1911g*, 99.

seems to me, however, to share the defect of traditional statements : this principle does not require recurrence, but is really concerned with the uniformity of a formula, as e.g. the Law of Gravitation, which gives uniformity, in the required sense, to the motions of the Solar System, though (so far as we know) no configuration ever recurs. What I meant by saying that the world is given as often as we please was, that all laws, and indeed all general statements, are hypothetical, i.e. of the form

$$\phi x \,.\, \supset_x \,.\, \psi x.$$

Hence if the laws of mechanics are true, we may feign any number of universes satisfying ϕx, and they must satisfy ψx. If we can't know whether $\phi x \supset \psi x$ without knowing whether ϕx is true, we have no true *law*. (I don't remember for certain, but I suppose this is what I meant.)

Jourdain responded to this outpouring of 2 January from Russell the next day in rather shorter style than usual : ' Many thanks '.

22. *February 1910–January 1911 : Jourdain on R*ss*ll, and Russell in reminiscence*

On 8 February Jourdain sent to Russell not only more manuscripts of his articles on prominent logicians, but also the manuscript of another article (*1911d*) entitled ' The philosophy of Mr. B*rtr*nd R*ss*ll '. This was a collection of witticisms on philosophy followed by an appendix of extracts from the writings of Lewis Carroll, in imitation of the structure of Russell's *The Philosophy of Leibniz*. Jourdain published two sections in *The Granta* in January 1907 as *1907b* (also on pages 178–179 below), and incorporated them also in this article. He seems to have published no more sections in that magazine, but its influence remained in the title, for it was their practice to replace with asterisks the vowels of the names of persons being satirised.

Unfortunately the correspondence provides little information about the preparation of the article, and thus does not much help us to determine which of the witticisms are due to Russell and which to Jourdain. The most substantial relevant document is the typescript in one of Jourdain's notebooks of part of a letter of 11 November 1904 from Russell to L. Hobhouse, of which a version appeared in the section on ' "Gedankenexperimente " and evolutionary ethics ' when the article appeared in *The Monist* (*1911d*, 502 (*1918b*, 88)) :

People who expect to discover what *is* good by inquiring what cannibals have *thought* good always surprise me. I sometimes feel inclined to apply the historical method to the multiplication table. I should get a statistical inquiry among school-children, before their pristine wisdom had been biassed by teachers. I should put down their answers as to what 6×9 amounts to, I should work out the average of the answers to six places of decimals, and then decide that, at the present stage of human development, this average is the value of 6×9. This process, it seems to me, would be as rational as the endeavour to base Ethics on Sociology.

Jourdain obtained this extract from Lady Welby,[1] and Russell referred to it immediately after his remarks about her on 20 October 1908 (see page 111 above) :

I don't in the least mind you having a copy of the letter to Hobhouse you speak of. I have no recollection of writing it, nor can I imagine what is in it, as I don't know anything about Höffding's ethics. I suppose they are evolutionary. Anything evolutionary always rouses me to fury.

We can also locate in the correspondence the passage in the article (*1911d*, 492 (*1918b*, 54)) dealing with Judge Russell. Jourdain had enclosed with his letter of 10 October 1906 a cutting (now lost), and described its contents as a case where

Judge Russell decides against a claim brought by an agent against his company for appointing another agent, on grounds that he was appointed ' the ' agent. It is rather ironical that a decision so contrary to logical grammar should be made by a Russell.

In his reply of 22 October, Russell had written : ' I am much amused about *the*, but ashamed of my namesake, whoever he is'. Finally, Russell's remark of April 1905 (page 45 above) on the role of ' the ' in the ontological proof of the existence of God appeared in the chapter on that word (*1907b* (page 179 below) ; *1911d*, 492 (*1918b*, 54)).

[1] For in the same place in Jourdain's notebooks are the typescripts of two letters dated 3 February 1904 and 25 November 1905, of identical format to the extract to Hobhouse from Russell (which Heath attributed to Jourdain in *1920a*, 173, incidentally). Jourdain has indicated that these letters are from Russell to Lady Welby, and that he received the typescripts from her on 22 September 1908. Parts of one were published in her *1931a*, 128, a volume containing extracts from some of her correspondence with both Russell and Jourdain (' a brilliant mathematician who died young ' ; p. 243). The originals are now in her *Nachlass* in York University, Toronto, Canada. The Russell Archives has a few letters of 1904 from her. The University of London Library possesses her library, including an interestingly annotated set of *The Monist*.

Jourdain also enclosed with his letter the latest issue of *The Monist*. It contained an article of his own, in which he repeated his remark of May 1907 (page 102 above) to Russell about the interpretation of existence by mathematicians as the property of an individual and by (Russellian) logicians as the non-emptiness of a class (*1910a*, 114–115). In his reply of 9 February 1910, Russell recalled once again the " existence " of individuals as expressed by his theory of descriptions :

> I hold that there is a sense in which we can say an individual exists, when it is *the* member of a class. You know the definition
>
> $$\mathrm{E!}(\iota x)(\phi x) . = : (\exists c) : \phi x . \equiv_x . x = c \quad \mathrm{Df.}$$
>
> This is what we mean when we ask if the most perfect Being exists, or if Homer existed : Homer = *the* author of the Homeric poems.

We recall from pages 85–89 above that in 1907 Jourdain had anticipated to some degree von Neumann's definition of ordinals. In the *Monist* article he used a somewhat similar procedure to prove the existence of cardinals and ordinals via the proven existence of smaller ordinals. Briefly, mathematical induction provides a progression of finite ordinals which as a class yields both ω and $\aleph_0$. Then all Cantorian ordinals of the second number-class are obtained by rearranging a progression. ' Hence the existence of ω_1 is proved ', Jourdain continued optimistically, ' and $\aleph_1$ is defined to be the cardinal number of terms *in* a series whose generating relation is of the type ω_1. Hence we can advance to ω_2 and $\aleph_2$, to ω_ω and $\aleph_\omega$, and so on ' (*1910a*, 115–118 ; compare *1906c*, 128).

As we noted on page 24 above, Russell had used an argument of this kind in 1904 in one of his alleged proofs of the axiom of infinity (where, as here, the defined objects were assumed at each stage to form a class). In his letter he recorded his acceptance of the axiom and objected to Jourdain's method for its mixing of types :

> Your chain of existence theorems for numbers I should now regard as fallacious, owing to the theory of types ; I now think existence-theorems for transfinite numbers require an ' axiom of infinity ' of which the simplest form is ' every inductive cardinal exists ' i.e.
>
> $$n \varepsilon \mathrm{NC\ induct} . \supset_n . \exists! n$$
>
> or, what is equivalent,
>
> $$n \varepsilon \mathrm{NC\ induct} . \supset_n . n \neq n + 1$$

or, if V is the class of all individuals,

$$V \sim \varepsilon \text{Cls induct.}$$

I think the theory of types will also make it awkward to define Cardinals as classes of smaller Cardinals.

Jourdain's articles on the history of logic, his article in *The Monist* and his covering letter provoked Russell to some comments about past and recent literature. Russell admitted that ' I do not know Boole's work well enough to offer any criticisms of your account of him ', and in exemplification of this point he said of Boole's *The Laws of Thought* that ' I don't possess his book ' but that ' my recollection is that the title " Laws of Thought " is merely due to acceptance of tradition, and that he means exactly what we should mean by " Laws of Logic " '. But Boole gave a psychologistic interpretation to logic in claiming that his system really did provide laws of thinking ; in fact, his own Boolean algebra was a metaphysical system, and only secondarily a method of solving logical equations. His whole attitude stood in marked contrast to Russell's objective standpoint.[1] A predecessor who was much closer to Russell in philosophical spirit was Leibniz, who appeared in another remark in this letter. Jourdain mentioned both Russell's *1900a* and Couturat's *1901a* on Leibniz in his article (*1910a*, 99), which provoked Russell to assess his own book very modestly against Couturat's : ' I don't think my book on Leibniz ought to be quoted as though it were of equal importance with Couturat's. His gave new material, and showed the place of the Logical Calculus in Leibniz's thought : mine did nothing comparable'.

A famous saying of Russell—' Mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true '[2]—was related to Russell's comment about an article against his work in the same issue of *The Monist* as Jourdain's article, written by its editor, P. Carus, and critical of Russell's logic :

> Yes, Carus is amusing. I agree cordially, however, on one point, namely when he says ' I am not sure that I have always rightly interpreted his humor '.[3] I like his solemnly proving

[1] On Boole's psychologism, see Musgrave *1972a*. In his article on Boole (*1910d*, 332–352) Jourdain did not stress this aspect of Boole's system, but in the one on Jevons (*1912g*) he described some of the modifications made by Boole's successors. The bound volume of Boole's correspondence which Jourdain borrowed for the writing of these two articles is now lost.

[2] Russell *1901a*, 84 (*1917a*, 59–60). The definition is a failure, as it is also satisfied by empirical sciences (if truth be taken to refer to ontology).

[3] Carus *1910a*, 63 ; see also pp. 49–51. The most notable part of this paper is the quotation on p. 45 of a letter to Carus from Peirce on the possibility of non-classical logics.

that when *he* talks mathematics he knows what he is talking about, and whether what he is saying is true. I am surprised that he thought my review of him favourable ![1]

On 11 February Jourdain sent a few manuscripts and notes to Russell for comment. In reply four days later Russell commented on one on Lady Welby : ' I am amused that you are become a writer on Significs in your own despite'. On 17 March Jourdain returned to mathematical questions, describing his current inclination to use definition by equivalence relations (which he called ' isoid relations ') rather than definitions of classes of classes in the Russellian way. This approach was close to Peano's principle of definition by abstraction, which we saw on pages 15 and 76 above earned Russell's distaste for its use of definition under hypothesis and for the lack of guarantee that the abstracted property was unique. But Jourdain now found Russell's approach guilty of non-uniqueness ; for example, irrationals might be defined as classes of segments, or classes of classes, of rationals.[2] He also pointed out that Russell's definition had been anticipated by M. Pasch in 1882.[3] When Russell replied from Ashley Gardens, Chelsea on 21 March he stated :

> My definition of Real Numbers seemed to me simpler and more convenient than the one given by the principle of abstraction ; this principle is useful as giving an existence-theorem, but there is no point in following it in all cases.
>
> I did not know what you tell me about Pasch. My knowledge of theories of irrationals is chiefly derived, so far as I remember, from Stolz's Arithmetik.[4]

Jourdain repeated his idea of defining cardinals cumulatively from smaller cardinals, and Russell again objected on the grounds of type mixing. Jourdain also suggested a definition of arithmetical division :

[1] Carus's *1910a* was a reply to Russell's review *1910a* of his *1908a*. On pp. 64–65 Carus reprinted Russell's review.

[2] Jourdain published these criticisms in *1913b*, 492, citing Russell's *1901c*, art. 1, ✳6.1 and ✳6.2. See also Jourdain *1910a*, 105–109.

[3] See Pasch *1882a*, 1–14, esp. pp. 4 and 11. Compare Jourdain *1913b*, 494 ; he may have been drawn to Pasch's work by its citation in Peano *1894a*, art. 12. Huntington also attributed the definition to Pasch in *1917a*, 53. Note also the analogy with J. S. Mill remarked upon in Kaufmann *1930a*, 82.

[4] See Stolz *1885a*, esp. pp. 97–104, and note Russell's citations of this source in *1903a*, ch. 34. Stolz mostly followed the Heine–Cantor approach to irrationals. In his letter Jourdain also mentioned Weierstrass's theory, which also involves classes of rationals but does not require an equivalence relation (see, for example, Biermann *1887a*, 19–20, or Mittag-Leffer *1920a*, 178—but *not* Russell *1903a*, art. 268, where the account of Weierstrass's theory is lamentable).

Why should we not define a ratio (p, q integers : couple of integers (x, y) a primitive idea, hypothesis used again for shortness) :

$$p/q \;.=.\; (x, y)\exists(p \times y = q \times x).$$

We could prove $\exists(p/q)$, which we can't if p/q were a relation.

Jourdain's intention was to fill the lacuna in Russell's account of the previous January on page 120 above of the arithmetic operations. He obtained the following new information from Russell :

> There seems to me no advantage in defining a ratio as a class of couples. If you put
>
> $$\mu/\nu = \hat{\rho}\hat{\sigma}(\rho \times \nu = \sigma \times \mu) \quad \text{Df}$$
>
> you have $\mu(\mu/\nu)\nu$, whence $\dot{\exists}!(\mu/\nu)$. This involves no new primitive idea.[1] (I have now accepted from Whitehead a new quantitive (non-arithmetical) definition of μ/ν, according to which it is a relation of vectors R, S which holds (broadly) whenever $\dot{\exists}!R^{\nu} \dot{\wedge} S^{\mu}$.[2] This enables you to take two-thirds of a pound of butter without an elaborate arithmetical détour.)

In *Principia Mathematica* the definition is exemplified as follows (vol. 3, 260) :

> A distance on a line is a one–one relation whose converse domain (and its domain too) is the whole line. If we call two such distances R and S, we may say that they have the ratio μ/ν if, starting from some point x, ν repetitions of R take us to the same point y as we reach by μ repetitions of S, i.e. if $xR^{\nu}y \;.\; xS^{\mu}y$.

The full-scale version of Russell's sketched definition is :

$$\mu/\nu \;.=.\; \hat{R}\hat{S}\{(\exists\rho, \sigma) \;.\; (\rho, \sigma)\ \text{Prm}\ (\mu, \nu) \;.\; \dot{\exists}!R^{\sigma} \dot{\wedge} S^{\rho}\} \quad \text{Df}, \quad (21.2)$$

where '(ρ, σ) Prm (μ, ν)' expresses the relation that ρ/σ becomes μ/ν when stripped of its common factors.[3] By this means we are saved such tedia as purchasing two pounds of butter and trading back in thirds (compare vol. 3, 407).

Russell's description of the definition as 'new' suggests that it replaced some earlier definition, which perhaps followed the lines of

[1] For (ρ, σ) is not assumed to be an ordered pair of integers in Russell's definition. With Russell 'primitive idea' could mean an axiom, a rule of inference or an indefinable.

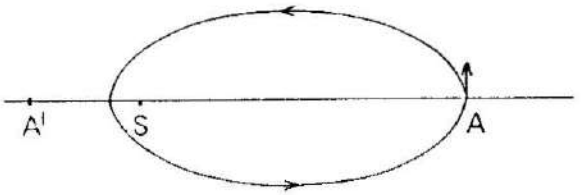
[2] This notation says in effect that the ν-th iterant of R and the μ-th iterant of S have intersecting domains and converse domains (see *PM*, ✳23.2 and ✳25.03).

[3] Compare *PM*, ✳302.03 and ✳303.01. ρ and σ are required in (21.2) by certain considerations of type theory, including the avoidance of the axiom of infinity (see vol. 3, 234 and 251–253). However, 'Prm' seems inessential, since (21.2) is satisfied also by $n\mu$ and $n\nu$ for any n. On the difficulties of defining distance logicistically, see Darbon *1949a*, 132–139.

his account of the previous January. His reference to Whitehead corroborates his later recollection in *1948a* that Whitehead was largely responsible for the latter part of *Principia Mathematica*.

Jourdain's final question in his letter of 17 March was rather different from usual :

> In your article [*1908b*] on Determinism and Morals in the Hibbert Journal, do you admit that the functions which our actions are of the time [sic] sometimes cease to be 1-valued ? Something as the position of a particle in Hertz's puzzle on determinism :



> from A project a man M vertically (with velocity v), under a force varying as inverse square from S. He describes an ellipse round S as focus. In the limit ($v=0$) [the] path is ASA ; but for $v=0$ [the] path is ASA'.[1]

Russell did not have too much trouble in solving this contradiction :

> No, I don't admit our acts are ever not a one-valued function of the time. Herz's [sic] puzzle is merely a case where
>
> $$\mathrm{Lt}_{h=0} f(a+h) \neq f(a),$$
>
> which is not really odd, though mathematicians think it so.

On 31 March and 9 April 1910 Jourdain sent Russell his manuscripts on the work of Frege and Peano respectively. On 10 April Russell returned the Frege manuscript :

> I return herewith your account of Frege. I have made one or two trivial notes in the margin, but otherwise found nothing to criticize. I agree with you that the account of Grundgesetze ought to be longer.[2] I have no books with me here, so I could

[1] By ' Hertz's puzzle ' Jourdain may have had in mind the problem of indeterminism at singular and turning points in the range of variation of a dynamical system (compare Jourdain *1912d*, 296–300). Despite receiving the advice of specialists, I have not been able to locate a precise reference in Hertz for this result. In fact, the puzzle has much earlier roots, as Jourdain might have noted from Bolzano *1851a*, art. 70.

[2] Jourdain meant to take this advice, for his article on Frege ends with an intention to continue the analysis of Frege's *Grundgesetze* (*1912a*, 269). However, it was never carried out, although he later published a joint translation of the opening parts of the first volume (for details see under Frege *1893a*).

not verify references or refresh my memory as to what Frege had said . . . Whitehead and I are at last getting the proofs of our book ; I suppose Vol. I will be published some time next year.

On 15 April Russell returned the Peano manuscript. Although he addressed his letter from Bagley Wood, he reported that he was still in London and without access to his books, but that he ' found nothing whatever to criticize ' in the article. Jourdain's covering letter had asked about the influence on him of Peano, and produced from Russell the following extensive reminiscences, especially welcome for their news about his discovery of Frege, the strengths of Peano, and the conversion of Whitehead to Peanoism :

I read carefully all the pages you specially asked me to look at. I think the introduction of ε was Peano's most important contribution, but the emphasis on implications with variables was also a great step. The reason I think it less important than ε is that the *practice* had always existed, e.g. in Euclid, and that Peano, though he emphasised such implications, did not give a very clear account of their nature.

I think the article should go to Peano. He is not touchy, so far as my experience goes, though he does not himself readily adopt improvements made by others.[1]

As regards your questions of autobiography, the first thing I *wrote* showing the influence of Peano was the article [*1901c*] in RdM [Revista di matematica], [volume] VII. I don't remember whether that was published before or after the article [*1901a*] in the *International Monthly* ; if after, the International Monthly was my first Peanesque publication.

I first read Cantor's work early in 1896 ; I was not then convinced that it was valid.[2] I then worked for some time at the principles of Dynamics ; I went to the Cavendish Laboratory, and I studied Clerk Maxwell [compare *1959a*, 39]. Gradually I found that most of what is philosophically important in the principles of dynamics belongs to problems in logic and arithmetic. This opinion was encouraged by my adoption of Moore's views in philosophy. In July–August 1900, Whitehead and I went to the Philosophical Congress in Paris. I was struck by the argumentative superiority of Peano and his disciples to all their adversaries, and became

[1] Jourdain sent his manuscript to Peano and incorporated Peano's remarks in the published version (*1912a*, 270–314). But his notebooks contain only one letter to Peano of 19 November 1903 and Peano's reply of five days later.

[2] Compare Russell *1967a*, 127. In *1896a*, 412, he claimed that the absence of an upper limit to the finite ordinals meant that the second number-class did not exist.

convinced that Peano's methods must lead to clear thinking.[1] I therefore then and there purchased from Peano Vol. VII No. 1 of RdM, and began studying it. I immediately read the whole of RdM, and whatever seemed important in the Formulaire. During September 1900 I invented my Logic of Relations; early in October I wrote the article which appeared in RdM VII 2–3; during the rest of the year I wrote Parts III–VI of my *Principles* (Part VII is largely earlier, Parts I and II wholly later, May 1902);[2] and in January 1901 I wrote the article for the International Monthly. In June 1901 I came upon the Contradiction about classes which are not members of themselves,[3] which occupied the bulk of my time and thought for the next five or six years.

I was led to buy Frege's Grundgesetze by an unfavourable review [*1895a*] by Peano in RdM, accusing Frege of unnecessary subtlety. The introduction struck me as admirable, but I could not understand Frege's use of Greek, German, and Latin letters, and I put him away for nearly two years, by which time I had discovered for myself most of what he had to say, and was therefore able to understand him. I must have first got Grundgesetze late in 1900.[4] I got an old copy of Begriffsschrift from James Ward, who could make nothing of it and had no use for it [compare *1967a*, 68].

Until I got hold of Peano, it had never struck me that Symbolic Logic would be any use for the Principles of mathematics, because I knew the Boolian stuff and found it useless. It was Peano's ε, together with the discovery that relations could be fitted into his system, that led me to adopt symbolic logic. I had already discovered that relations with assigned formal properties (transitiveness etc.) are the essential thing in mathematics, and Moore's philosophy led me to wish to make relations explicit, instead of using only ε and ⊂. This hangs together with my attack on subject-predicate logic in my book on Leibniz [*1900a*, arts. 10–12].

[1] Compare Russell *1959a*, 65–66, and *1967a*, 144–145. For more precise information on Peano's inspiration of Russell, see p. 114 above.

[2] Compare Russell *1959a*, 72–73, and *1967a*, 145. The manuscript of *The Principles* is in the Russell Archives and repays a detailed study.

[3] This date for the paradox was also given by Russell in *1944a*, 13 and also in *1956b*, 26. But in *1959a*, 75 he dated it in the spring of 1901, and in *1967a*, 147 in May. Jourdain described some later (and now lost) correspondence with Russell in Jourdain *1913h*, 146 (correcting *1912b*, 143), reporting that Russell discovered Cantor's paradox for himself in January 1901 (compare Russell *1901a* (*1917a*, 69)) and his own paradox in the following June.

[4] Russell meant only the first volume of Frege's *Grundgesetze*, as the second was not published until 1903. Note that he possessed the volume during part of the period of preparation of *The Principles* and of the discovery of the paradox, but apparently did not read it.

I read Schröder on Relations [*1895a*] in September 1900, and found his methods hopeless, but Peano gave just what I wanted. Oddly enough, I was largely guided by the belief that relations must be taken in *intension*, which I have since abandoned, though I have not abandoned the notations which it led me to adopt [compare pages 95–96 above]. The only part played by Cantor's work was that I tested my logic of relations by its applicability to Cantor.[1] But geometry was also much in my mind.

There was no more correspondence until 18 November, when Jourdain sent Russell for comment the proofs of his review for *Mind* of a book *1910a* by one Hastings Berkeley entitled *Mysticism in Modern Mathematics*. This was the kind of book which foundational studies inspire but do not deserve : objections mostly based on misunderstandings which overlook the genuine criticisms that could be brought. Wittgenstein found the perfect phrase in a review *1914a* of another work of similar calibre : ' the worst of such books as this is that they prejudice sensible people against the study of Logic'.

Jourdain said that he had marked some passages for comment. Maybe one of them was the place where he showed his awareness of Russell's conflation of use and mention by urging logicians to ' follow Frege ' and distinguish between, for example, Paris and ' Paris '.[2] But Russell, who had recently sold his house at Bagley Wood and was staying at the time in Whitehead's house in Carlyle Square, London, did not pick up the point in his reply of 22 November. Instead, he reported the following interesting comments from Whitehead :

I agree with all your own views as expressed in your review, but I have not read Berkeley except here and there in bits. Whitehead, who has reviewed him [*1910a*], thinks your review too favourable, though, like me, he agrees with all your own views, in particular with your remarks on himself. From what I have seen of Berkeley, I too should have thought that you read more into him that is really there ; but I have not read enough to have

[1] Russell added the footnote : ' I was with Whitehead when he was writing an article for American Journal of Mathematics on applications of Cantor to Logical Equations, and I persuaded him that my logic of relations would greatly facilitate his work. This produced his conversion (January 1901) '. The pertinent work is Whitehead *1902a*, to which Russell contributed sect. 3. Perhaps the process of conversion had already started, and this was the culmination.

Jourdain noted on this letter from Russell that on 16 April he sent a note of thanks.

[2] Jourdain *1911b*, 91 ; compare *1911d*, 486 (*1918b*, 22), and *1912a*, 262. He also wrote a shorter review *1911a* of Berkeley's book, and Hardy wrote a critical review *1910a*. Other books of the period of similar calibre include Richardson and Landis *1915a* and *1916a*, and Shaw *1918a*.

much right to an opinion. Whitehead tells me to say he does not and never did agree with Boole's principle of uninterpretable Symbols, and in his Universal Algebra tried to get rid of it, but was misunderstood by Berkeley owing to Berkeley's not realizing that by 'mark' he meant a mere shape on paper.[1] I myself do not think uninterpretable symbols can be usefully employed; but this gets obscured by the use of *variable* symbols of operation.

Boole's 'principle of uninterpretable symbols' was his view that the union of two classes x and y was defined only if x and y were disjoint (see *1854a*, 66–70). Its rejection by Russell and Whitehead relates to their avoidance of definition under hypothesis. The problem with variable symbols of operation which Russell reported to Jourdain probably concerned the requirement that no operation within the range of the variable be allowed to impose a restriction of its definition which was not needed by the other operations (in *Principia Mathematica*, see ✳38).

This exchange of letters also included an enquiry from Jourdain of whether a proof by H. Lebesgue of the Heine–Borel theorem in Lebesgue's *1904a*, 104–105 used the multiplicative axiom. Russell confessed himself too busy to investigate the matter, though he reminded Jourdain that 'in general the multiplicative axiom is required to proof [sic] that the sum of $\aleph_0$ classes of classes has $\aleph_0$ terms'. But the possible need of such axioms in proofs of that time related to the manner of determination of the covering intervals. One underlying factor was the failure always to discriminate between a limit-point of a set P (defined as the point towards which an infinite sequence of members of P converged) and an accumulation point of P (defined by the property that at least one member of P lay within each of its neighbourhoods). If a closed set is defined in terms of limit-points, then an axiom of choice is needed to prove the theorem; but if the other definition is used, then it is unnecessary. The use of the reflexive or non-inductive senses of infinity is also involved.[2]

Russell also asked if Jourdain could review for *Mind* P. Natorp's *Die logischen Grundlagen der exacten Wissenschaften*. 'Stout sent it to me asking me to send it to you if I didn't do it', he continued, 'I have kept it some time expecting to do it, but find myself too busy.

[1] See Berkeley *1910a*, 74–77; he cited Whitehead *1898a*, 3–5 and 37, but see also pp. 10–12. Whitehead partly accepted Berkeley's criticisms on p. 239 of his *1910a* (a practically unknown article, by the way, and notable for its stress on the philosophy of the variable).

[2] Jourdain discussed such problems in *1906f*, *1910c*, *1915c*, *1922a*, 244–246, and also in notes dated 22 October 1908 in his notebooks. See also, for example, Sierpinski *1918a*, art. 4.

It is Kantian, and (I think) foolish'. Jourdain must have agreed to the request, for he published a (fairly critical) review *1911f* in *Mind* in the following year (see also his *1912g*).

In December 1910 the first volume of *Principia Mathematica* was published (see my *1975c*), and on 24 December Jourdain acknowledged the receipt of a complimentary copy and enclosed a list of misprints and comments. No copy of this list survives, but Russell's reply on 2 January 1911, from his brother's house at Chichester, dealt with a few of them (compare section 28 below). The most interesting concerned the interpretation of implication in the section on 'Primitive ideas and propositions'. For the primitive propositions included 'What is implied by an elementary proposition is true' (✳9.12; compare ✳1.1). One crucial word is 'implied', which we have noted in Russell might refer to implication, inference or the conditional. Another word is 'anything'; not necessarily propositions, as Russell stressed in these rather vague comments to Jourdain about his symbolic language:

> I no longer think it significant to deny
>
> $$x \supset q,$$
>
> where x is not a proposition. I think that, strictly, one ought not to use a single letter for a proposition, but always some such symbol as ϕx. But so long as this is remembered, it is not necessary always to do what strictly ought to be done.[1]

Jourdain's other points to Russell about the first volume must have concerned the definition in ✳24.01 of the universal class V as $\hat{x}(x=x)$ and of $\vec{R}`x$ in ✳32.13 as $\hat{y}(yRx)$ (x and y interchanged here). For Russell defended his definition of V thus:

> The definition of $\hat{x}(\phi x)$ depends on ϕ, and if ϕ is variable, so is $\hat{x}(\phi x)$. But when we put
>
> $$V = \hat{x}(x=x) \quad \text{Df},$$
>
> ϕ is constant, and thus V is constant.

However, he admitted that the definition of $\vec{R}`x$

> is just as wrong as the definition of Cnv`R, but I remarked over

[1] This passage is a summary of *PM*, vol. 1, 129, though Russell had already expressed the point (which again involves the avoidance of hypotheses) in *1906b*, 161–162. Compare p. 113 above, and also Frege's introduction of the conditional in *1893a*, art. 11.

Cnv'R that in the Introduction I was going to ignore this [vol. 1, 32]. The wrongness does not consist in being under a hypothesis, but in the fact that ' Cnv ' or ' $\breve{R}$ ' ought to be defined by itself.

In his covering letter Jourdain asked for an offprint of Russell's *1901a* in the *International Monthly*. Russell replied :

> I am sorry I have no copy of my article in the International Monthly, and I have not reprinted the article, as I found I should have to spoil the jokes to bring it up to date. The only person I know of who has a copy is Miss Jane Harrison, Newnham College, Cambridge. I dare say she would lend you her copy. (She is the archaeologist.)

Jourdain may have obtained a copy later, for while commenting on Carus *1910a* in his *1912b*, 149, he mentioned some printing errors in the article. Russell seems to have communicated them in letters which are now lost.

In his letter Jourdain also reported some recent correspondence with Mrs. Boole about his article (*1910d*, 332–352) on her husband's work. By then the old lady was nearly eighty and had become steadily more idolatrous about Boole's Boolean algebra, as Jourdain found out :

> Mrs. Boole, who didn't approve of my taking George's name in vain, has now told me that I acted ' under higher guidance than hers, or even George's '. That is very comforting. But more exhilarating is the fact that she has thought me worthy of a Christmas present of her work on the ' Fun of Algebra '.[1]

Around 1905 Russell had nearly become involved in editing Boole's manuscripts,[2] so he knew the problems :

> I am much relieved to hear that in writing on Boole you acted on ' higher guidance '. I had heard Mrs. Boole was indignant at the suggestion that symbolic logic was the subject of her husband's book.

[1] M. Boole *1909a*. There is no correspondence with Mrs. Boole in Jourdain's notebooks.

[2] According to the letters to Russell from O. T. Falk (now in the Russell Archives) and Russell's replies (in private possession) Russell declined a request to become editor and recommended Couturat ; but the project never came to fruition. Boole's *Nachlass* is now kept in the Archives of the Royal Society of London ; parts were surveyed in Hesse *1952a*.

23. *January 1911–August 1917 :* The Monist *and the War*

We have now reached the end of Jourdain's notebooks. Fortunately we have been able to cover the main period of Russell's logical work, for by 1911 most of the manuscript of *Principia Mathematica* was at the press, and the remaining sections were mainly Whitehead's responsibility. But it is still frustrating to have so little of their later correspondence ; less than a score of letters and carbon copies are preserved in the Russell Archives, and all but two are after the summer of 1917.

An event in which Jourdain and Russell both participated was the International Congress of Mathematicians at Cambridge in August 1912. Russell was one of the convenors for the sub-section on the philosophy and history of mathematics. He did not deliver a paper, but gave a short opening address *1913a* in which he praised Cantor's work. By contrast, Jourdain gave four papers (*1913a–1913d*) : one on equivalence relations, one on the theory of functions, and two on historical developments. By then his paralysis had advanced to such an extent that he had to use a wheelchair, but he made a contact there which was to be of great importance for his later relationship with Russell. Carus, the editor of *The Monist*, attended the conference and after meeting Jourdain invited him to be the English editor of the journal (see Heath *1920a*, 175).

1911 and 1912 were transitional years in Russell's life, when he eased himself out of mathematical logic and tried to enter fully into philosophical writing. But they are particularly difficult to document, as important sources are missing. We know of the delay from January to May 1911 of the publication *Principia Mathematica* caused by the need to remove the axiom of infinity from cardinal arithmetic (see pages 106–108 above), but only from a few letters from Whitehead that Russell preserved. Russell's correspondence with Ottoline Morrell (see page 115 above), which was particularly frequent during this period with the passionate flowering of their affair after the break-up of his marriage with Alys, indicates that in June 1911 he approached unsuccessfully the Cambridge University Press with some project from Jourdain. It also shows that he wrote several works with or at least for her, but two principal ones are now missing. One was a book on the philosophy of religion called *Prisons*, of which a chapter was published as his *1912b*, and the other was an autobiography. The latter would be particularly valuable for the history of logic, for his logical studies would surely have been given a more detailed discussion than the rather perfunctory treatment they received in his later autobiographical writ-

ings.[1] It might also shed some light on the curiosity concerning Wittgenstein in 1909 on which we stumbled on pages 114–115 above.

Whatever the extent (if any) of that 1909 contact may have been, the relationship between Wittgenstein and Russell began to develop only in the autumn of 1911, when Wittgenstein came up to Cambridge and began to criticise Russell's current philosophical work. In particular, Russell was writing a book on epistemology during the summer of 1913, but Wittgenstein's criticisms caused him to abandon it. He published six chapters as articles in *The Monist*, but the rest was left in manuscript.[2]

As English editor of the journal, Jourdain must have been involved not only in these articles but also with Russell's other contributions. In particular, he translated into English an article *1911b* on logic by Russell in French, and published it in *The Monist* with Russell's revisions as Russell's *1913b*. Up to 1917 Russell published his *1912c* and *1915c* in that journal. It was owned by the Open Court Company, who also published both *Open Court*, to which he contributed *1916a* and *1916b*, and American editions of two of his books: *Our Knowledge of the External World* in 1914, and *Justice in War-time* in 1916.[3] Jourdain was involved a little in the preparation of the former work, for not only is there an acknowledgement of help to him[4] but also his secretary is the subject of the famous story told by Russell later that when the 'shorthand typist' entered his room, he immediately knew how the book should go and 'dictated [it] in a completely orderly sequence from that moment until the work was finished'.[5] In fact, Russell's memory was at fault (see Blackwell *1974a*), for his correspondence with Ottoline Morrell shows that he worked on the book (which was delivered as the Lowell Lectures at Harvard in the Spring of 1914) all through the latter half of 1913. The work that came to him in this form seems to have been the paper *1914a* entitled 'The relation of sense-data to

[1] Russell *1956a* is almost silent on even personal aspects, and *1967a*, ch. 5 is little better. *1959a*, chs. 3–8 is the best source, but it is not historically detailed.

[2] The articles are *1914c* (three chapters), *1914d*, *1915a* and *1915b*; the rest of the manuscript is in the Russell Archives. The whole work is studied in some detail in Blackwell *1974b*.

[3] The surviving early files of the Open Court Company are kept in the Morris Library, Southern Illinois University, Carbondale, U.S.A., but they contain no significant material on the Russell–Jourdain relationship. However, they are valuable for Jourdain's relations with Carus (see Eames *1975a*).

[4] Russell *1914b*, 165. A letter of 18 November 1913 from Jourdain survives in the Russell Archives, and relates chiefly to this assistance. Jourdain published a very complimentary review *1914a* of Russell's book.

[5] Russell *1967a*, 210; perhaps the secretary was the Miss Harwood acknowledged in Jourdain *1912e*, 415. She also typed out Wittgenstein's 'Notes on logic' (see McGuinness *1973a*). In Sarton *1923a*, 129, Mrs. Jourdain said that he produced enough work at that time 'to keep two typists busy all day'.

physics ', which Jourdain's secretary duly typed out for him. She also typed out corrections to the Lowell Lectures and took down notes of some of his conversations with Wittgenstein.

The Open Court Company also published a good deal of material for Jourdain during this period. He prepared a number of editions or translations of others' work which appeared under their imprint, especially an edition of Boole's *The Laws of Thought*[1] and an edited translation of Cantor's paper of 1895–1897 on set theory.[2] In *The Monist* they also published many articles by him relating to Russell's work ;[3] and one of these is the only article of this period on which the surviving correspondence sheds any particular light. He was preparing a second article on ' The philosophy of Mr. B*rtr*nd R*ss*ll ', and on 26 August 1911 he wrote from Cambridge for some more material for it. Later Russell recalled that the articles were compiled by Jourdain from their ' frequent meetings ' (Denonn *1943a*, 16 ; Schilpp *1944a*, 797), which is quite possible for this second article ; for while we have seen that their meetings were infrequent during the time of preparation of the first one, Jourdain lived in Cambridge from 1911 to 1915 and Russell was also in residence there for some periods. Russell's diaries that survive in the Russell Archives indicate meetings with Jourdain on 12 November 1911 ; 11 February, 14 March, 19 May and 12 July 1912 ; 19 February (see page 115 above), 4 March, 13 May and 8 October 1913 ; and 27 February 1914.

The second article was published as Jourdain *1916a*. The subject matter of the section on the ' Objective validity of the " laws of thought " ' (p. 125 (*1918b*, 15)) had been mentioned in the exchange of 28–30 April 1909. Jourdain reported that in correspondence with W. J. Greenstreet he had ' mentioned the servant who doubted whether " A is either out or not-out " applied to the real world '. Russell replied : ' I am much amused that you found such a prompt use for my joke about the maid '. Possibly the joke arose during the conversation of the previous week, when we saw on pages 112–113 above that the law of excluded middle had been discussed. Further, Jourdain's remarks of July 1905 about

[1] The details are given under G. Boole *1854a*. Jourdain did not live to produce the edition of Boole's other logical writings that he intended ; but such an edition was prepared by R. Rhees as Boole *1952a* (including some of the manuscripts mentioned on p. 137 above), which the Open Court Company published in America together with a reprint of Jourdain's edition of Boole's *1854a*.

[2] Cantor *1915a*. Jourdain's lengthy introduction *1915d* was based on his earlier historical studies *1906h*, *1908f*, *1910e* and *1913j*.

[3] See Jourdain *1911e*, *1912b*, *1912f*, *1913h*, *1915c*, *1915e*, *1916a*, *1916b*, *1917b*, *1917d* and *1919a*. Correspondence with Russell is indicated in *1912b*, 149, and *1913h*, 146. Among his other papers elsewhere, see especially *1917a*. He also served from 1917 on the board of the *International Journal of Ethics*, but did not write articles in it. After his death he was succeeded by his sister Margaret.

insects and garden-rollers on page 54 above found their way into a discussion of ' The humour of mathematicians ' (*1916a*, 45–46 (*1918b*, 74)).

With his letter of 26 August 1911 Jourdain also enclosed ' all I can find of the notes Frege sent me on my Account of his work ', and they are now in the Russell Archives. But he could not have examined his notebooks carefully—for the missing pages are there ! Russell published this letter in his autobiography (*1967a*, 217), though it is difficult to see what the uninformed reader would make of it.

When the War started in 1914 Russell largely abandoned philosophical work. But he did not sever his connection with Jourdain, for in addition to articles in journals on which Jourdain served there was the common bond of pacifist sentiments. Let this section end with a poem which Jourdain sent to Russell at some time during the holocaust :

The War and Christianity.

All Christians see at once how most absurd
It is, and even impious, to dare
With finite mind to read into His word
A literal meaning that it cannot bear ;
As with His famous : ' Blessed are the meek ;
To him that smiteth turn the other cheek'.

Nay, let us rather look beyond the ill
We see, or think we see, in war's behests ;
The eye of faith will help us, and the will
To hear the deep notes of our interests.
All work for death is cursed, the thoughtless say,
Yet even Bishops find munitions pay.

No angel yet has ever been observed
To help a Briton in a British trench,
But Britons were by horseback angels nerved
In a case unforseen by Sir John French.
Praise God for sending them uninterceded,
But praise Him more since they are rarely needed.

The war has issues far beyond our ken :
Men fall like grass before the August scythes ;
It raises hell on earth ; but then
It also raises the poor parson's tithes.
The dead lie thick beneath a foreign sod,
But Mr. Bottomley believes in God.[1]

[1] ' Mr. Bottomley ' refers to H. Bottomley, Member of Parliament, founder of *John Bull* and advocate of the War Cause. Jourdain expressed his anti-war sentiments in *1915a*, and his dislike of Christianity in *1919i*.

24. *September 1917–September 1919 : a bitter ending*

During 1917 the War was at its worst, both in the trenches and at home, and pacifists such as Russell became subject to attacks by ' patriots '. On 4 September Jourdain wrote to him expressing concern about the recent attempted assault that he had suffered (see Russell *1956a*, 32–34). In reply the next day from his brother's house in Gordon Square, London, Russell recounted his own version of the incident and then reported his current intention to resume philosophical work :

> To pass to other topics, I am bringing out a set of collected philosophical essays, published by Longmans, to be called ' Mysticism and Logic, and other Essays ' [*1917a*]. Also I shall be giving lectures on the philosophy of mathematics during the autumn in London. Wildon Carr is getting these up.
>
> My interest in philosophy is reviving, and I expect before long I shall come back to it altogether. I want to write (1) an Introduction to modern logic, (2) a new edition of ' The Principles of Mathematics ', and I want to do a lot of work, applying mathematical logic to the principles of physics along the lines of my book on the ' External World '. If I had done all this, I could die happy.
>
> What is your position in relation to the ' Monist ' now ? Is it the same as it was ? Does the censorship interfere with you as regards transmitting manuscripts to America ?

Jourdain replied the next day from Fleet, Hampshire (whither he had moved after his marriage in June 1915) :

> I have long hoped that you would write some introduction to modern logic. I wish you would publish in form of articles in the Monist first of all. I remember you had an idea at one time of an article or set of articles ' What is logic ? '. This sounds exactly the thing for the Monist. Besides I want to know badly what logic is.
>
> In revising ' The Principles ' is there any hope that you would reprint with added notes in square brackets showing how your views have been modified ? I feel strongly that this ought to be done.

Jourdain's desire ' to know badly what logic is ' was doubtless an

intentional hint that Russell had so far failed to explain his conception of logic. Russell's reply of 11 September showed that the point had been either missed or deliberately ignored :

> I think that if and when I can manage to write the ' Introduction to Modern Logic ' that I spoke of, I should like to publish it first of all in the form of articles in the ' Monist '.
>
> I do not think that in revising the ' Principles ' I can adopt the plan you suggest. The whole of the first part would need to be entirely and completely re-written ; so would the third part ; and the second part would need very tremendous alterations.

Before writing this reply Russell had received another letter from Jourdain, written on 8 September and asking quite different questions :

> I am engaged in writing for Mind a belated review of the Principia. Will you tell me whether you *started* in mathematical logic from Cantor (the work of 1895 and 1897 I suppose) or from Peano. I had the impression that it was *Cantor* as mentioned in Couturat's book of 1897 [sic : *1896a*] on infinity, and your seeing that Cantor's work was closely connected with the foundations of arithmetic (a thing which the Huns do not yet see) first excited your interest. I should like to know this and any other details you can remember as a general introductory word on your ' place in history '.

Jourdain did not publish a review of *Principia Mathematica* in *Mind* (in fact, no one did for the first edition) although he published several elsewhere.[1] But Russell answered this letter in his reply of 11 September and supplied further reminiscences of the start of his logical work :

> As to my ' place in history ', I would have you to know that I am not yet dead. I may yet end as Emperor of Abyssinia, or President of the German Republic. Nevertheless, with this proviso I am willing to answer your questions.
>
> I first came across Cantor through the work on Hannequin ' Hypothèse des Atomes ' [*1895a*], which I had to review for ' Mind ' [*1896a*], and then again through Couturat's ' Infini

[1] For the first volume of *Principia Mathematica*, see Jourdain *1911c* and *1913f* ; for the second, *1912c*, *1913e* and *1915b* ; and for the third, *1914b* and *1918a*. Some reviews mention matters discussed in the correspondence, such as the conflation of use and mention, definition under hypothesis, the various senses of ' existence ', and the non-constructibility of ω_ω and $\aleph_\omega$.

Mathématique' [*1896a*, reviewed in Russell *1897a*]. I read all the articles in the 'Acta Mathematica' [especially Cantor *1883c*] carefully in 1898, and also the 'Mannigfaltigkeitslehre' [*1883b*]. At that time I did not altogether follow Cantor's arguments, and I thought he had failed to prove some of his points. I did not read the articles of 1895 and 1897 until a good deal later. I first came across Peano in 1900 at the Paris Congress of Philosophy, and it was he who started me on mathematical logic. It was only after mastering his work that I discovered how to treat arithmetic by mathematical logic. I wrote Parts III to VI of the 'Principles' in the autumn of 1900. In the spring of 1901 I discovered the contradiction about the classes which are not members of themselves.[1] Later on I discovered that most of what I had done had been already discovered by Frege.

Jourdain acknowledged this letter with a short note of the same date.

The lectures on the philosophy of mathematics that Russell had mentioned on page 142 above took place that autumn as a course on mathematical logic at Dr. William's Library in Gordon Square,[2] near his brother's house where he then lived. They were followed by a course of eight lectures on 'The philosophy of logical atomism', which were delivered at the same venue each Tuesday from 22 January to 12 March 1918. Russell seems to have spoken verbatim or from notes rather than full manuscripts,[3] but the second course of lectures was taken down by stenographers and typed out. He must have arranged with Jourdain to publish them as articles in *The Monist* before issuing them in book form (though they were not the 'Introduction to Modern Logic' of which he had spoken to Jourdain on page 142 above), for on 4 February Jourdain wrote concerning the typescripts of the first lectures :

> I like your lectures very much indeed. May they go to the Monist exactly as they are ? You can alter them when you want to publish them in book form. Please give us [the Open Court Company] the first chance of the book when you are going to publish it.

A week later he wrote again, apparently acknowledging the receipt

[1] Note the date given this time for the paradox, and compare p. 133 above.

[2] More precisely, in the building owned by Dr. Williams's Trust and often known then also as 'University Hall'. The Trust tell me that they are concerned with theology, and would not have mounted the lectures. Probably they just hired out the lecture room on the ground floor.

[3] This and several other points in the remainder of this section draw on Russell's correspondence with Allen and Unwin, of which both sides are available in original or photocopy in the Russell Archives.

from Russell of the checked typescripts, and announcing the proposed manner of publication in pairs and method of payment :

> Many thanks for lectures. Payment will be made directly I hear the first two have appeared (two will appear together). Will you send me 3 and 4 corrected to hold in readiness ? Nothing further than 1 and 2 shall appear until the first two are paid for. I am very grateful to you for letting me have the lectures.

In the spring of 1918, after the lecture course was finished, Russell was convicted of publishing unpatriotic pacifist literature (see his *1968a*, 32–33). Jourdain wrote on 9 April, firstly in support of Russell's appeal and then about a technical matter in the first lecture :

> I hope that your appeal will be successful. If not, nothing that I say would be of any real use to you, but you know that you have all our sympathy and very many of our thoughts. I hope that conditions will not be so bad as to prevent your thinking about pleasant things like logic, and if so I wish you would think out what exactly are the relations between propositions and symbols. I cannot feel at all satisfied with what you say in your lectures on this point [*1918a*, 504 (*1956a*, 185)]. Surely a proposition is not a symbol, for I do not see what a symbol can be except either a simple or a complex name. No doubt a proposition is not a name for a fact, but you seem to maintain that it is a complex name for a fact (i.e., a name which is not itself arbitrary, though some of the constituents are arbitrary, being simple names). There are some other points which I should like to discuss in future and I have hope to write them out intelligibly. Just at present work of different kinds seems to crowd in on one.
>
> Do let me have a line if your appeal is successful.

The matter under discussion was Russell's rejection in his lecture of the idea that a proposition can be the name of a fact, a rejection which he acknowledged as an influence of Wittgenstein (*1918a*, 507 (*1956a*, 187)). The consequent treatment by Russell of simple and complex names is not very clear,[1] and it is a pity that there is no record of his reply to Jourdain. In fact, there is no more surviving correspondence for nearly a year. During that time Russell went to prison for six months for his sins, and Jourdain prepared a reorganised and augmented version of ' The philosophy of Mr. B*rtr*nd R*ss*ll ', which Russell approved for Allen and Unwin to publish as a book (the Open Court

[1] Compare, for example, Pears *1967a*, esp. ch. 4 with Ayer *1971a*, 34–48 and 81–91.

Company publishing it in America).[1] Jourdain's editorial preface is of relevance to the correspondence, for it begins : ' When Mr. B*rtr*nd R*ss*ll, following the advice of Mr. William James, again " got into touch with reality " . . . ' (Jourdain *1918b*, 3). This must be an allusion to Russell's quotation from a letter of William James when writing to Jourdain on 20 October 1908 (see page 112 above).

The lectures on logical atomism began to appear in four consecutive issues of *The Monist* ;[2] but the payment did not come to Russell as quickly as he would have liked, and on 11 March 1919 Jourdain had to inform him that there would be more delay because of the death (in February) of Carus, the editor of *The Monist* and manager of the Open Court Company.[3] He also referred to the multiplicative axiom, and in a way which suggests that it had once again become the subject of discussion in letters which are now lost :

> I am rather disappointed that you speak of the multiplicative axiom as insoluble : that appears to me sheer dogmatism. I am quite sure my proof is valid. Neither your nor Whitehead's remarks seem to touch the point of the thing because the very essence of it is *actual construction* by induction of a class of direct continuations which determines a chain that is of the sort that cannot be continued. The construction avoids Zermelo's principle because *many* classes of direct continuations are constructed *simultaneously*. I enclose a short paper which I hope you will read. The fact that people either will not read or refuse to understand an idea which I deliberately think is valid—and in which, owing to my belief that I am not quite an idiot,[4] I still say I have avoided carefully arbitrary selections—has reduced me almost to despair.

Jourdain had *another* proof of the well-ordering principle which allegedly avoided axioms of choice. He published versions of it in various places ; indeed, the paper which he sent to Russell may well have been one of them (or its manuscript).[5] This proof harked back

[1] The bibliographical entry for the work in Schilpp *1944b*, 761 explains that ' This book is intended as a joke ', which in the sense of its ch. 39 is a joke of the second order.

[2] Russell *1918a*, *1919b–1919d* (*1956a*, 177–281 : on p. 177 Marsh states that the articles appeared in *three* issues of *The Monist*, and he fails to cite any of them).

[3] Jourdain wrote an obituary notice *1919j* for Carus. Sarton *1923a*, 128 seems in error with the date of death of Carus, and also in saying that Jourdain succeeded Carus as general editor.

[4] Jourdain added the footnote : ' Here by this I mean (1) that I know quite well that picking out *a* chain involves [the] multiplicative axiom ; you will see how I avoid this in [the] enclosed ; (2) I do not think " all trespassers will be prosecuted " ⊃ some will be '.

[5] Jourdain *1918c*, *1919b*, *1919c*, *1919e*, *1919f*, and the posthumous *1922a* which Mittag-Leffler published in *Acta Mathematica*. He also reported on his proof in letters to the editor of *Nature* (*1918d*).

to his attempt of December 1905 on pages 60–61 above to construct a maximal class. In the meantime much progress had been made in these directions, and Jourdain was probably influenced by some of them. Zermelo's second proof of the well-ordering principle by the construction of θ-chains (*1908a*, art. 1) was probably one source, though Zermelo explicitly assumed an axiom of choice. Hausdorff may have been another influence, for he presented proofs of the well-ordering principle in maximal-seeking forms which he seemed to regard as avoiding both such axioms and transfinite induction.[1] F. Hartogs's proof *1915a* that Zermelo's axiom of choice and the well-ordering principle were each equivalent to the trichotomy law (two cardinals are equal, or one is greater than the other) was also influential, since it drew on the construction of well-ordered segments of ordinals.

In his new proof,[2] Jourdain called a well-ordered sub-class of a class M a ' chain ' of M. The class C_M of chains could be regarded as the union of its sub-classes of chains of each order-type γ, but the aim of the proof was to re-arrange C_M into other kinds of sub-classes of chains, called ' K-classes '. A K-class corresponds in purpose somewhat to Zermelo's θ-chain ; given an ordinal γ, the K-class K_γ contains chains for all smaller ordinals and also all the segments of each of its members (when it would be a ' direct continuation ' of its segments in the sense of Jourdain's letter above). Now a property of a well-ordered class is that each segment is also well-ordered and is therefore a chain. Therefore K_γ contains all the segments of some particular chain p and so uniquely determines p.

Now if some K-class contains all the members of M, then the well-ordering principle is proved. If not, its manner of construction ensures that any chain p_γ of order-type γ is included in *every* chain of type $\gamma+1$, so that no choice process is needed to progress from p_γ to $p_{\gamma+1}$. In particular, if γ has no immediately preceding ordinal then we can still obtain a chain p_γ ' by an induction which in general is transfinite, and which does not depend on Zermelo's principle of arbitrary selection ' (as Jourdain wrote optimistically in his *1922a*, 240). Eventually the class M must be exhausted (and so be well-ordered) ; for otherwise it would be possible to find a sub-class of M in one–one correspondence with the paradox-generating class W of all ordinals.

[1] See Hausdorff *1914a*, esp. pp. 133–139. See also *1962a*, 65–68 and 338–339.

[2] I have introduced some notation into this version of the proof, which largely follows Jourdain *1922a*, 239–240 together with the idea of a ' direct continuation ' from the earlier proofs.

Axioms of choice can be proved from the assumption that two classes in one–one correspondence are either " consistent " or " inconsistent ". Cantor had such an assumption in a letter to Dedekind (see Cantor *Papers*, 444), and von Neumann in effect had a proof in *1925a*, art. 4. But Jourdain does not seem to have followed this approach.

Apart from any other point, Jourdain offers us only his faith that the step to p_γ when γ has no immediate predecessor does not involve an infinite selection. Unfortunately we do not have Russell's reply to the new proof, but presumably he pointed out the assumption. At all events, from this time (the spring of 1919) onwards Jourdain showed signs of retaliation and invective against Russell.

We recall from page 142 above that in September 1917 Russell had promised to Jourdain that if he were ever to write his ' Introduction to Modern Logic ' he would publish it first as articles in *The Monist*. In the end he wrote his *Introduction to Mathematical Philosophy* in prison in 1918 and published it in March 1919 as a book (*1919a*) from Allen and Unwin without prior appearance in *The Monist*. Now in April someone who saw an advance or review copy of the book informed Allen and Unwin that passages of it corresponded closely to passages in the lectures on logical atomism. Russell denied the charge (with complete justice) to Allen and Unwin, and reported his suspicion that their informant must be Jourdain himself. He must also have written to Jourdain, for on 4 April Jourdain admitted his guilt[1] and answered Russell's latest request for payment for the lectures :

> I think the trouble arose from my asking Allen and Unwin whether they would proceed against the Monist for copyright because some parts of the articles are very like parts of the books. In particular the theory of descriptions is almost exactly the same even to the instance to what will appear in the April Monist.[2] I quite agree that most of the rest of the articles are very different in point of view and only one or two passages are like.
>
> With regard to the payment naturally I will do my best about the thing but you must remember that Mrs. Carus through whom all payments are made is very upset by the death of Carus and I hardly like to bother her for some time. However, if you think it is necessary I will do so.
>
> I hope you will write soon about the multiplicative axiom : I feel as much about it as the man you quote from Browning about δε.

Jourdain's final remark is an allusion to the opening of the chapter in Russell's *Introduction to Mathematical Philosophy* on descriptions :

[1] In his letter of 11 March Jourdain prefaced the passage quoted on p. 146 above about the ' insolubility ' of the multiplicative axiom with : ' Many thanks for your book '. Perhaps this was an advance copy of the *Introduction to Mathematical Philosophy*, in which the axiom is so viewed (ch. 12) ; but if not, it might have been Russell's *Mysticism and Logic*, which Jourdain reviewed viciously in *1919a* and rather less so in *1919h*.

[2] Compare Russell *1919c*, lecture 6 with *1919a*, ch. 16. Jourdain grossly exaggerated the similarities.

'like Browning's Grammarian with the enclitic δε, I would give the doctrine of this word [namely, 'the'] if I were "dead from the waist down" and not merely in a prison' (*1919a*, 167). It must indicate Jourdain's awareness of decline of his health, which was doubtless the cause of his steadily more desperate feelings about his new proof. On 8 May he asked if Russell had received a 'simplified proof' sent on 24 April (but now lost). Russell must have answered this letter, for Jourdain replied on 24 May as follows :

> I can only now reply to your letter of May 9 as I am only just out of bed after an illness. I told you that as Carus had died it was both awkward for me and inconsiderate to Mrs. Carus to worry her about payments for the Monist just at present. You said that you understood this, and I may remark that firms like G. Bell and Allen and Unwin accept the necessary delay. Your suggestion that you will examine my proof when the Monist pays you strikes me as rather unworthy. It is practically asking me to bother Mrs. Carus specially for you in order that I may get some approval from you of what I have written. But you seem to have overlooked another aspect of the case : by refusing to look at my proof you are deliberately shutting your eyes to truth. It is not to the point to say that you have tried hard to make out what I wanted to say : I cannot think you have done so because I expressed perfectly clearly that it seemed impossible to solve the problem of well ordering by the usual process A, so that I used an indirect process B. Your only reply was to point out the difficulties which everybody knew perfectly well in the process A.
>
> When I wrote out my suggestions more than a year ago I imagined that Hartogs had provided a certain necessary step and did not repeat what I thought he had done.[1] Whitehead, you, and Hardy, all pointed out the gap in my proof : I examined Hartogs again, found he had not done what I thought he had, and supplied the link. Whitehead and Hardy simply repeated what they said at first, thereby proving that they did not read my new paper with understanding. When I pointed this out Hardy left my letter unanswered and Whitehead said he had no time to read any more of my stuff. I do not then think that other objections are valid against what I wrote ; while your objection was stated above. It does not matter to me that you refuse either to accept or disprove a certain principle : I should have thought it mattered to you.

[1] A reference to Hartogs *1915a* ; Jourdain explained his claim in more detail in *1922a*, 248–249. The revisions to which Jourdain alludes may be the (minor) differences between the version of the proof in *1922a* and the earlier versions.

It is a pity to treat this principle as if it would be doing me a personal favour to examine it. Personal questions are surely out of place in such a matter. But since you have thought fit to plead poverty as a reason for not doing what you seem to think is a personal favour I may perhaps mention that rather more than five years ago when you wanted some money badly I had to take the advancing of £100 to you as a responsibility on myself, since at first Carus would not contemplate paying £100 for the Lowell lectures.[1] I did this out of a sense of duty or friendship although I could not afford it : my total earnings were then £200 a year and my total private income £12.

Russell must have replied at once, for Jourdain wrote again on 27 May in a more conciliatory tone :

Am sorry that I assumed you did not want to read my proof until I had got the money out of Mrs. Carus. Of course I do not object to anybody for thinking my proof invalid : what I object to is that Whitehead and Hardy seem to have made no effort to see the point of the construction of a chain (which I carried out in a perfectly non-arbitrary manner throughout) ; and then either did not reply or wrote that they could not trouble to read it. If the thing were true, it seems to me that it is one of the most important things in mathematics ; if it is not true, really good reasons ought to be given which do not rest on bias. You say that none of you have any bias against me, but in that case how do you account for this. In 1902 I told Whitehead of a new definition of a continuous function I had found.[2] In 1903 you said in your Principles [art. 304] that continuity of functions is not a wholly ordinal notion. As my definition showed that it *was*, I published this definition in 1905 [*1905e*, 182–189] and sent you and Whitehead copies (exact details are in Mind for April 1919 [*1919d*]). In 1912 Whitehead publishes a definition which is in principle the same as mine,[3] and you, on page 107 of your Introduction, give all the credit to

[1] Namely, Russell's *Our Knowledge of the External World*, which was published in America by the Open Court Company. It seems that Jourdain never recovered his £100 and Russell was not paid for his lectures ; for in the Russell Archives there is a letter of 28 November 1919 from H. F. Kite, the solicitor for Jourdain's estate, requesting the return of £100. Russell has written on it : ' If any money is owing, it is from [the] Open C[our]t Co[mpany], not from me '. Russell never published in *The Monist* again ; the contribution listed in Schilpp *1944a*, 778 is in fact Weiss *1932a*.

[2] If this communication was made in writing, then there is no trace of it in the notebooks.

[3] *PM*, ✳234. Russell had described this section of *Principia Mathematica* in detail in his letter of 2 January 1910 (see pp. 119–124 above).

> Whitehead. The affair is not of great importance but it does not seem to me quite fair. With regard to the multiplicative axiom there is no point in the whole construction at which an arbitrary selection is made ; so that if my proof is not valid it must be for some quite other reason. So please make allowances for me : it is so beastly to work at the thing and get results which nobody seems able to see and the only arguments against it miss the point.

Although Jourdain was now trying to take a more reasonable line, he had already broken the friendship beyond repair. In the article in *Mind* which he mentioned, for example, he wondered if Russell's current interest in psychology was becoming ' so strong that he will devote the rest of his life to a history of the Church or a treatise on animal behaviour '.[1] Elsewhere he wrote that of two ' mathematical logicians ' called ' W.' and ' R.' W. was ' bored with well-ordered series ' and had made irrelevant criticisms of his new proof, while R. saw difficulties in it and so must visualise ' mathematical logic as a very different thing from logic '.[2]

Jourdain's illness may have become so serious that it was affecting his sense of judgement. By the autumn he was so ill that his wife had to reply on his behalf on 11 September to another request by Russell for payment for his lectures on logical atomism :

> My husband cannot attend to your letter because he is dangerously ill with heart attacks. I can only say that naturally the Open Court Company do not repudiate their debts : it is merely a question of not bothering Mrs. Carus at a very anxious time. Neither you nor Dr. Whitehead have taken any trouble about my husband's own serious work so I do not quite see the force of your complaint.[3]

At this time Russell was sharing a farmhouse with J. E. Littlewood outside Lulworth, Dorset, not far from Jourdain's home at Fleet,

[1] Jourdain *1919d*, 168 ; pp. 167–170 are vicious throughout. His remark on Russell's interest on psychology was presumably a reference to Russell's *The Analysis of Mind*, which he began in prison (see Russell *1968a*, 152) and published in 1921.

[2] Jourdain *1919f*, 383–384. He repeated many of these accusations in *1920a*, which Russell repudiated in his own *1920a*.

[3] Mrs. Jourdain added the postscript : ' Prof. Mittag-Leffler has the question in hand so you need not trouble further about it '. In his correspondence with Mittag-Leffler (see p. 6 above) the previous June Jourdain had expressed his intention to submit a paper on the well-ordering principle to *Acta Mathematica*, but in the end it came only with the notebooks in May 1922 (pace Mrs. Jourdain's recollection in Sarton *1923a*, 131–132). When Mittag-Leffler published it as Jourdain *1922a*, he added a footnote to the first page saying that he did not want to receive any more manuscripts of this kind!

Hampshire.[1] As Jourdain lay dying, he could think only of his latest proof of the well-ordering principle, and he called for Russell and Littlewood to come and here it. Littlewood went over with Dorothy Wrinch as a joint representative for Russell and recalled the occasion later :

> When he was dying he sent a request to a party staying in Lulworth, including Russell, Dorothy Wrinch, and myself, asking someone to come to listen to his final proof. Dorothy Wrinch and I went, and he gave his ' proof ', fallacious as usual. (The subject is a great trap, many eminent mathematicians have assumed the axiom when they thought they were not.) . . . I said that a new point was involved, and I would have to consider it closely and at length. Then he burst out : ' My dear fellow, you know perfectly well that you can see whether a proof is right or wrong in 5 minutes'. Strange outbreak of complete rationality ! We were booked to leave the following day, and that was all.[2]

On 24 September Jourdain wrote to Dorothy Wrinch about the visit and the new proof :

> I am very sorry you would not take the expenses as I thought it was quite the right thing to do : it was very pleasant to see you both.
>
> Of course I should like to see Russell if he thinks there is any chance of agreeing. If only he could go with you as far as you can follow me that at any stage the K's are transformed into a set of K-classes of which each has the same ordinal number of terms, that would I think be enough, but I do not want to distress him should he think that it is necessary for my peace of mind to agree, where he does not really do so, because I am somewhere near the end of my tether.[3]

The next day Miss Wrinch wrote to Russell advising him to go down to Fleet and see him.[4] Russell sent a telegram which presumably proposed a visit, but it was too late. Jourdain died on 1 October 1919, and his wonderful correspondence with Russell ended like this :

[1] See Russell *1968a*, 95–96. The rest of this section is based on my *1973a*, 10–11 as modified by my *1973b*.

[2] Extracts from a letter of 22 November 1969 from J. E. Littlewood to J. M. Rollett, quoted by permission of author and recipient.

[3] This letter is among Jourdain's letters in the Russell Archives. There is also a letter of 17 October 1917 from Jourdain to Miss Wrinch in this collection.

[4] This letter is among Miss Wrinch's letters to Russell in the Russell Archives.

September 26, 1919

Dear Mr. Russell,

Your telegram came just too late to arrange for Philip to see you. He is now quite unable to talk or see anyone, but just lies in a semi-unconscious state. Why didn't you make an effort to come a little sooner. You have made him so unhappy by your inability to see his well-ordering. You are the only person he wanted to see and talk with months ago.

Your truly

Laura Jourdain.

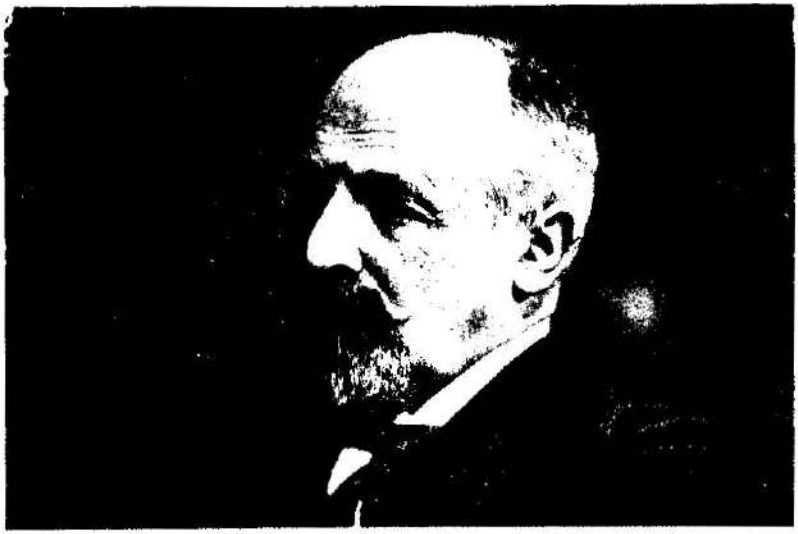
Plate 1

This photograph comes from Alys Russell's collection, and is reproduced by kind permission of Mrs. Barbara Halpern from a copy made available by R. W. Clark. Russell is undoubtedly the figure on the right, but the identity of the other person has not been established. At first glance it appears to be Whitehead, but relations and acquaintances of his are convinced that it is not. Despite considerable research among photographs of the time, I have not been able to put forward any alternative identification. Russell seems to be in his late twenties, which dates the photograph as of around 1900. The shutter suggests a house of French design, and the shadows indicate a sunny day. An event in Russell's life which may be consistent with these three factors is the International Congress of Philosophy held at Paris in August 1900; but there seems to be no means of deciding whether or not the photograph was taken then.

Plate 2

There seem to be very few surviving photographs of Frege. Two are almost identical, and were doubtless taken at the same sitting. I have seen them reproduced only as facial portraits (in, for example, the frontispieces to van Heijenoort *1967a* and Frege *1969a*); but we appear to have here the full photograph (of which the frontispiece to van Heijenoort *1967a* is a part). The artificial pose, complete with a learned-looking volume which is not being read, is typical of the formal portraiture of professors made in connection with some university celebration. Frege's university, Jena, celebrated its 350th anniversary in 1908, when Frege was in his late fifties. This would be a plausible estimate of his age in this undated photograph; for, according to Carnap *1963a*, 5, he looked older than he was at that time.

This copy of the photograph belonged to Russell, and has been generously lent to me for reproduction by Lady Russell. It seems to be a copy of an original, for it is mounted on a card which carries the name and address of a Cambridge photographic studio. The signature underneath the photograph has been cut from a letter and stuck on the card, and someone has written Jourdain's name on the back. None of Frege's letters to Russell (now in the Russell Archives) or of his surviving letters to Jourdain (now in the notebooks) is so defaced, nor does any information seem to be available of the presentation of the photograph by Frege or of its copying. But it seems plausible that Frege gave the photograph to Jourdain, who had a copy made for Russell and stuck on it the signature of a (now lost) letter from Frege. Possibly the copy was made in the period 1911–1915, when both Russell and Jourdain lived in Cambridge; Russell certainly possessed it by 1916 (see Lenzen *1971a*, 6).



Herzlichen Gruß von
Georg Cantor
Naumhof, 13 April 1905

Plate 3

A signed photograph of Cantor, sent to Russell presumably in 1905 and reproduced here with the kind cooperation of Lady Russell. The circumstances of the taking of the photograph are interesting. Cantor suffered occasional attacks of mental illness (see page 157), one of which lasted from September 1904 to March 1905 (see my *1971b*, 371). During its course he conceived an essay, in the form of a discussion between a master and a student, in which he asserted that Jesus Christ was the natural son of Joseph of Arimathea. After leaving the clinic he stayed for some time with his sister at Naumhof (near Leipzig) and not only wrote up the essay but also had this photograph taken. He published the pamphlet himself under the title *Ex oriente lux* and sent it out, possibly with the photograph, to friends and acquaintances. Russell may have received a copy (see his *1967a*, 217) as well as the photograph, and the Youngs received both (the photograph in a smaller copy ; see my *1971b*, 371).

Jourdain was sent the manuscript of the essay, and was asked to translate it from German into English. He fulfilled the request (see my *1971a*, 124–125), though the translation is lost. Cantor's manuscript, however, has survived in Jourdain's notebooks.

Plate 4

Philip Jourdain, captured in a rather self-conscious pose typical of portrait photography of that time. Little seems to be known about the origins of the photograph, but it shows Jourdain apparently in his early twenties; around the time, therefore, that he began corresponding with Russell. It is reproduced by kind permission of Jourdain's widow, Mrs. L. Breglia, from a copy lent to me by J. M. Rollett, who received it from Mrs. A. E. Heath.

Epilogue

25. *The relationship between Russell and Jourdain*

In the course of this correspondence we have followed Russell's progress from *The Principles* to *Principia Mathematica*, and found many new details about the journey that he took. We saw him abandon the type theory of *The Principles* and spend over two years on methods of solving the paradoxes which did not necessarily assume a type structure. Then the theory of definite descriptions of 1905 suggested to him a new approach in the substitutional theory, with its rejection not only of classes and relations but also of propositional functions and free variables, and a reliance only on individuals and propositions. The theory was short-lived, however, and from the autumn of 1906 onwards we traced a line of relatively purposeful development in the construction of the system that was published in *Principia Mathematica*. All along the way we have also found examples of the mathematical problems for whose solution the logic was designed, especially in set theory, transfinite arithmetic and the structure of the real line.

A central problem in Russell's investigations was the nature of classes and their status in mathematical logic. He carried out many pioneering analyses of the relationship between classes and their members, and studied a number of distinctions which have remained important in the philosophy of mathematics and logic ever since. The distinctions between an object and its unit class, and between membership and inclusion, had already been examined by people such as Peano and Frege; but Russell surpassed them all when he discovered his paradox, for he showed with that result that a naive and unrestricted use of the membership relation led to inconsistency. Thus he was drawn to a problem which perhaps only Cantor had studied in any detail before him; to find a criterion for the determination of classes. His search led him to examine in detail the abstraction of classes, their relationship with propositional functions, classes as one as opposed to classes as many, intensions as opposed to extensions. Unlike Cantor, he did not see the solution of the paradoxes in the limitation of the size of classes, but tended rather to try to reduce the complexity of the propositional functions used to determine them. Eventually he adopted the

vicious circle principle as the criterion of excess complication, and committed himself to a type theory in which he saw the principle playing a major role. Classes there took members only from the immediately lower type. Further, they were subsumed under propositional functions, a view which he had tended to adopt in earlier years when a plausible system was still far from attainment.

As part of his process of subsuming classes, Russell defined them contextually as (effective) subjects of propositional functions. Contextual definition avoided assigning an isolable referent to the defined term, and he used the device also in defining descriptive expressions as part of his theory of definite descriptions. He was deeply interested in problems of denoting from an early stage of his work, and devoted much effort to the relationship between propositional and denoting functions. By means of contextual definition he was able to define denoting functions in terms of propositional functions instead of leaving them as indefinable. He was attentive to other aspects of definitions, including their well-formation and the avoidance of hypotheses.

A feature of his theory of definite descriptions was the view that descriptive phrases are not names of anything, a view which involved him in the distinction between naming and referring. Another problem concerned with this distinction was the status of propositions. For a number of years he allowed them to be abstract nameable objects, but then he found himself in difficulty reconciling this standpoint with a correspondence theory of truth.

Russell's most important contribution to the more technical aspects of mathematical logic was his development of the logic of relations. Previously relations had been treated only as an extension (albeit an elaborate one) of Boolean algebra, but Russell applied the new ideas that he had learnt from Peano to introduce a comprehensive logic of relations into Peanesque logic. By this means he extended the range of applicability of logic to mathematics to such an extent that *Principia Mathematica* was to be a unique landmark in the axiomatic presentation of mathematics.

The correspondence between Russell and Jourdain has featured all the issues summarised above, treating some of them very extensively and providing new historical information about them all. It has also shown how often Russell had to change his mind on many of the problems which beset him. This is surely one reason why *Principia Mathematica* is in some respects a rather confused work; Russell had tried so many alternatives during the fallow years that he could not always assess them as relevant or irrelevant to the final system adopted. But another reason is his failure to distinguish use from mention. In Frege we see this distinction usually observed carefully, and Russell

read Frege in detail and brought his work to the general attention of the scientific world. But Russell did not show the same respect for use-mention distinctions, probably because of the strength of his conversion to Peanoism, where such distinctions were also not maintained. He had simply got himself so much into the Peano style of working—with its numerous technical felicities, and potential for application far greater than that of any rival, Frege included—that he could not criticise himself out of it. An interesting piece of documentary evidence corroborates this view; in his notes on Frege's work (now in the Russell Archives) Russell converted Frege's symbolism into Peano's.

Let us turn now to another prime influence on Russell, one which is too little recognised but which comes out wonderfully well in the correspondence—Cantor. Although Russell saw Cantor primarily as a technical influence (see page 134 above), the techniques and even some of their philosophy permeated many of the problems that he discussed with Jourdain: irrational numbers, the definitions of continuity, the relationship between cardinals and ordinals, the provability of the well-ordering principle, the series of ordinals and the definitions of number-classes and cardinals from them, Cantor's paradox of the greatest cardinal, and the whole question of the scope and limits of set-theoretic techniques in mathematics. Similarly, large parts of *The Principles* and *Principia Mathematica* are devoted to Cantorian problems, either directly from Cantor or through intermediate figures such as Peano. And the influence is not just technical or mathematical; for without doubt a factor of prime significance in Russell's logicist thesis is that when he claimed that pure mathematics was reducible to logic, he included Cantorian set theory within the mathematics involved and devoted much of his detailed exegesis to it.

Unfortunately in his autobiography Russell spoilt the impression of his opinion of Cantor by prefacing his letters from Cantor with the remark that Cantor 'spent a large part of his life in a lunatic asylum' (*1967a*, 217). This is a singularly offensive remark to Cantor's descendants (as they have told me) and, moreover, wrong (see my *1971b*, 368–369). A truer view of his feelings for Cantor is surely given by his *1913a*, *1958a* and *1959a*, for he held Cantor in such esteem that until his last days he kept signed photographs of Cantor and Frege on the walls of his study (see Plates 2 and 3).

The following feature of Cantor's influence on Russell is particularly noteworthy; Russell's rejection of non-classical logics. The debate between classical and non-classical logics if often treated today as rather divorced from mathematics;[1] but when Russell considered the problem

[1] Rescher's advocacy of non-classical logics (*1969a*, ch. 3) seems based on a mixture of conventionalism and a plea for tolerance, while Quine's rejection of them (*1970a*,

(especially in the debate with MacColl) he had to bear in mind that logic must include among its duties the foundation for Cantorian set theory. As we saw on page 24 above, Cantor invoked classical logic, including the law of excluded middle, to claim in formalist vein the consistent construction of his transfinite numbers as the justification of their existence. He also used it in his belief that every class was well-defined, for it then followed that the membership function of a class would be two-valued ; an object would belong to the class or not. He made this point quite explicitly, in a passage which should be taken as an important background influence on Russell :

> I call a class . . . *well-defined*, if on the grounds of its definition and in consequence of the law of the excluded third, it must be considered as *internally definite* . . . whether any object belonging to the same range of the concept [Begriffssphäre] belongs as an element to the considered class or not.[1]

The discovery of the paradoxes was to shake everybody's belief in well-definability and thus to provoke an examination of the foundations of classical logic. But for Russell (as for Cantor) it must have seemed hopeless to abandon the law of excluded middle and still preserve Cantorian set theory.[2]

Another aspect of Cantor's influence on not only Russell but also all foundationalist mathematicians of the time was the axioms of choice and their ramifications. Today these axioms are not widely regarded as a controversial matter ; with their numerous equivalents, they are significant in a number of branches of mathematics. But in the early 1900s they were an issue of the highest importance. The introduction of a new axiom into an area of established and respectable mathematics is always a traumatic experience, but in this case further difficulties were involved : the scope and limits of their applications ; the discovery of equivalent versions ; the philosophical acceptability of their non-constructive character (and the possibly different grades of constructibility and thus acceptance which different equivalents showed) ; surprising results, such as non-measurable classes and the paradox of the sphere, obtained with their aid ; and above all for mathematicians such

---

ch. 6) relies similarly on a mixture of aesthetics and a plea for conservatism. A variety of conceptions of logic is analysed in Corcoran *1969a*.

[1] Cantor *1882a*, 115 (*Papers*, 150). An extensive study of Cantor's life and work, using many new manuscript sources, is under preparation by J. W. Dauben (see his *1972a*). The surviving Cantor *Nachlass*, which both Dauben and I have used, has recently been placed in the Archives of the Göttingen Academy.

[2] This statement is meant to describe Russell's apparent line of thought rather than to decry the development soon afterwards of intuitionistic mathematics.

as Jourdain, the dispensability of such axioms altogether. These axioms and their consequences may be regarded as characterising a whole new phase in mathematical analysis and set theory,[1] in the form of a critical analysis of unintended assumptions in the *multiple*-limit analysis that was developed by the pupils of Weierstrass,[2] just as Weierstrass himself instigated research into the hidden assumptions in the *single*-limit analytical techniques of the early nineteenth century.[3]

A few lines ago Jourdain was mentioned for the first time in this epilogue. We have to be greatly indebted to his excellent critical powers for the quality of this correspondence, for it was the penetration of many of his questions which caused Russell to write back at such length and detail. The same quality is apparent also in parts of his published writings on Russell's work; he was one of the first to notice Russell's looseness over use-mention, the multiple senses of 'existence', and so on.

On the other hand, his progress in research work was very halting and fitful as compared with Russell's. We saw him develop his theory of "consistent" and "inconsistent" classes in order to avoid the paradoxes, and in the process grope towards some later ideas in abstract set theory and anticipate in part Hausdorff's theory of regular and singular ordinals. We also saw him attempt a cumulative definition of ordinals which was to be developed in a comprehensive form only years later by von Neumann. Above all, we found several determined efforts to eliminate axioms of choice from mathematics, and on the way anticipate slightly the later interest of Hausdorff and others in maximal principles as an alternative to choice methods. But each of these endeavours seems to peter out in unresolved questions, to stop at places which others (especially Hausdorff) independently took as starting points. In particular, his determination to eliminate the axioms changed from a viewpoint to a dogma. In the end it was to destroy his relationship with Russell, a relationship which over the years had embraced not only mathematical logic and the foundations of mathematics but also mutual friends and acquaintances, publications by Russell with organisations with which Jourdain was concerned, and common pacifist sentiments.

Having considered the scope of their relationship, let us look now at its limits. In the first place, a correspondence is intrinsically limited in

[1] The history of the axioms of choice and their significance still needs writing (a project being undertaken by G. H. Moore). Some details are provided by Sierpinski *1918a*, *1928a*, ch. 6, and *1958*, chs. 6 and 16, Church *1927a*, and Cassina *1936a*. Rubin and Rubin *1963a* list the equivalents whose discovery form an important part of the history, while Jech *1973a* gives the axioms a modern setting.

[2] By far the best accounts of the developments from Weierstrass to the time of measure theory are Hawkins *1970a* and Medvedev *1974a*.

[3] This point is the main theme of my *1970b*, as is stressed in my *1975d*.

its range, for rarely does it deal thoroughly with *all* aspects of the correspondents' mutual interests. It comes into being only on the few dozen occasions or so that the participants sit down to write to each other, and so is likely to cover only questions of the most immediate concern. It is an oblique form of contact ; it lacks the personal touch, the projection of ideas by a person as well as the ideas themselves, or the possibility of the simple, clear characterisation of a problem which may occur only during a discussion and perhaps never at the times of preparation of a letter or of a published work. To learn at a high creative level from a master, a pupil needs to feel his presence and to share some of his life, to catch the illuminating points which may not yet be in a form suitable for the public but which can stimulate the interest and imagination of the alert listener.

Jourdain did not obtain that degree of personal contact from Russell. He heard Russell's lectures during the winter of 1901–1902, and indeed probably learnt of foundational questions in mathematics from them. He also saw Russell quite a few times in later years, but the closeness of the relationship was not sufficient for him to learn from Russell at the level which he could doubtless have achieved. This is a particular misfortune, for Russell's letters reveal him as the kind of out-going, flowing personality for whom such a relationship would have been a valuable stimulus. Indeed, the unpublished Russell sheds a new light on his personality for those acquainted only with his published writings : a tremendous warmth of soul and generosity of spirit, to complement the public ingenuity of intellect and sharpness of wit.

Yet Russell's published work does reveal more of his personality than perhaps he intended. I close this section with a point which has not received attention in the secondary literature on Russell and may not have been noticed by Russell himself. During a few months in 1901 and 1902, just before his correspondence with Jourdain started, he experienced three events of great importance to his later career. He realised from observing the pains of Mrs. Whitehead that mankind is born to suffer ; he discovered from his paradox that mathematics was not securely founded ; and he found from his personal feelings that he no longer loved his wife Alys. Thus during this short period he suffered three major set-backs, to his natural optimism, his intellectual aspirations, and his personal stability ; and in his autobiography (*1967a*, 146–151) he described these developments in an *intermingled* way that cannot be accidental even if it were unconscious. In particular, he refuted each of the three principles of life which he announced in the prologue to that book : ' Three passions, simple but overwhelmingly strong, have governed my life : the longing for love, the search for knowledge, and unbearable pity for the suffering of mankind ' (*1967a*, 13).

I hand over now to Russell and Jourdain. The next two sections contain papers of theirs which they published during the time of their correspondence and which complement or illuminate some of the points made in their discussions. These papers appeared in relatively obscure publications and so have received an undeserved measure of neglect, from which their reappearance here may at last save them.

26. *Bertrand Russell : ' Sur les axiomes de l'infini et du transfini '*

In March 1911 Russell visited Paris and delivered two lectures on his logical work. One of them was published in the *Revue de Métaphysique et de Morale* as ' L'importance philosophique de la logique mathématique ' (*1911b*). Russell seems to have written it directly in French, for Jourdain translated it into English for publication in *The Monist* as Russell *1913b*.

The other lecture, ' Sur les axiomes de l'infini et du transfini ', was delivered to the Société Mathématique de France on the same day, 22 March 1911.[1] It was published in the *Comptes Rendus* of the Society as *1911a*, but the journal was poorly distributed and often did not reach the libraries who subscribed to the Society's better-known *Bulletin*. Recently the Society reprinted the back issues of the *Bulletin*, and incorporated its *Comptes Rendus* in consecutive pagination of each volume ; but the paper remains little-known. Russell did not preserve his manuscript (though fortunately he kept an offprint, for I read it from a photocopy), and so it is not clear whether he wrote it in French or had it translated afterwards. But I have translated it for publication (by permission of the Society) in this section, because in dealing with the axiom of infinity and the multiplicative axiom it relates closely to many of the discussions between Russell and Jourdain.

As usual, I have enclosed any editorial additions to the text within square brackets. I have also converted Russell's references into the system already in use in this book, and I have added footnotes of my own to clarify certain textual points and to correlate significant passages with the appropriate parts of *Principia Mathematica* (though I felt it unnecesssary to produce a detailed interpretative analysis). I have also indicated the pagination of the paper in the margin, both for its original publication in the *Comptes Rendus* for 1911 (when it occupied pages 22–35), and also for its recent republication with the Society's *Bulletin* (where it takes pages 488–501).

[1] See the letter of 9 March 1911 to Russell from P. Montel, now in the Russell Archives. According to his *1967a*, 203, Russell gave a third lecture in Paris, at the Sorbonne (see Russell *1911c*).

22/488 Lecture by Mr. Bertrand Russel [sic], professor at Trinity College (Cambridge), *On the axioms of the infinite and of the transfinite :*

We know that pure mathematics can be expressed and proved entirely in terms of ideas and axioms of logic. This thesis is obvious to anyone who considers the nature of deduction. When we deduce, we say : x possesses a certain property, therefore x possesses some other property. Now, if we can know this, it is certain that the two properties must have a formal connection, which allows the transformation of properties into variables, and the assertion : any two properties having such-and-such a formal connection are such that one of them implies the other. We have there a proposition of logic. In fact, any mathematical proposition becomes a proposition of logic by transforming into variables a sufficient number of constants that the proposition can contain.

I shall give a geometrical example of this procedure. The Geometry of infinite spaces (which includes that of Euclid) can be developed as a theory of the relation *between* among points. We define *points* : any term[1] y which is between two terms x and z. Let us put ϑ for *between*. We define the line (x, z) : the terms y such that $y\vartheta(x, z)$ or $x\vartheta(y, z)$ or $z\vartheta(x, y)$ and x and z. We have axioms such as :

$$y\vartheta(x, z) \,.\, \supset \,.\, \sim x\vartheta(y, z),$$

$$y\vartheta(x, z) \,.\, z\vartheta(y, w) \,.\, \supset \,.\, z\vartheta(x, w), \ldots .$$

We can then consider any relation ϑ having these properties, and we have some logic.

However, we can secure the reduction to logic only by avoiding asking with too much insistence if there exist objects which verify the hypotheses whose consequences we are considering. It happens sometimes that we could consider such objects *a priori* ; for example, we could construct *a priori* a class having any finite number of terms. (As a matter of
23/489 fact, this construction is only a *a priori* on admitting as *a priori* the axiom that there exists at least one object, or some equivalent.)[2] But the majority of existence-theorems (which, moreover, are not necessary for the *truth* of other theorems, but

[1] Russell used the word ' term ' in a very wide sense in this article, to refer to objects such as points or members of a class as well as to words. The definition of *points* here is rather cryptic; indeed, as G. Spencer Brown has suggested to me, the only interpretative comment possible for these first paragraphs of the lecture is ' sic '.

[2] A problem on Russell's and Whitehead's minds in March 1911 ; see pp. 106–108 above.

only for their importance) require known quantities which are not purely logical. In pure Mathematics, two existence-axioms give pretty nearly all the existence-theorems that one could desire. These two axioms are :

1. The axiom of infinity ;
2. The multiplicative axiom, in other words, *Zermelo's axiom.*

These two axioms cannot be demonstrated by logic, and, in my opinion, they do not have intuitive evidence. However, I would like first of all to explain their nature and their consequences, before examining the reasons that we might have for admitting or denying their truth.

The axiom of infinity may be expressed as follows : *If n is any finite cardinal number, there exist classes consisting of n individuals.* Here the word *individual* is opposed to class, function, proposition, etc. ; in other words, *individual* signifies *being of the actual world, as opposed to the beings of logic.* In order to develop this notion, it would be necessary to explain the theory of logical types, which I do not want to do ; the lacunae which result from this silence will doubtless be visible to my listeners.

Let us call i_n the class of all the classes[1] of n individuals. Then, given the axiom of infinity, the series[2]

$$i_0, i_1, i_2, \ldots, i_n \ldots$$

forms a *progression*, that is, a series whose ordinal[3] number is ω (according to the language of Cantor). Thus we find that the axiom of infinity is the necessary and sufficient condition for the existence of progressions, that is, for the existence of series of the form

$$x_1, x_2, \ldots, x_n, \ldots, \text{ad inf.}$$

This axiom is therefore the necessary and sufficient condition for the existence of the smallest transfinite ordinal number ω, and of the smallest transfinite cardinal number $\aleph_0$.

24/490 Given the existence of $\aleph_0$, we also have the existence of $2^{\aleph_0}$, since this number is the number of classes contained in a

[1] In this sentence, as well as in some other places in the paper, Russell used the words ' classe ' and ' ensemble '. But there seems to be no discrimination in meanings of the words, and so I have translated them both by ' class ' unless a distinction is required for stylistic reasons.

[2] I translate ' suite ' throughout as ' series ', in conformity with Russell's practice, although the modern term is ' sequence '.

[3] Russell wrote ' cardinal ' here by mistake.

class having $\aleph_0$ terms. In the same way we have the existence of $2^{\aleph_0}$, $2^{2^{\aleph_0}}$, and so on; and it is easy to show that all these numbers are different, and that they form a series in order of increasing magnitude.

Here, then, is a first infinite series of transfinite cardinal numbers.

Given the existence of ω, we also have the existence of all the other ordinal numbers which are formed by changing the order of terms in a progression. For example, we have the series

$$1, 2, 3, \ldots, n, \ldots,$$
$$2, 3, 4, \ldots, n+1, \ldots, 1,$$
$$2, 4, 5, \ldots, n+2, \ldots, 1, 3,$$
$$2, 4, 6, \ldots, 2n, \ldots, 1, 3, 5, \ldots, 2n-1, \ldots.$$

Thus we obtain a certain class of ordinal numbers, namely, the class of ordinal numbers of series whose terms are the finite integers. Cantor has shown that we can arrange these ordinal numbers in order of increasing magnitude. Therefore these numbers themselves form a well-ordered series, whose ordinal number is greater than any number which appears in the series. We shall call this ordinal number ω_1. We also show easily that the cardinal number of terms in a sequence of type ω_1 is greater than $\aleph_0$; following Cantor, we shall call it $\aleph_1$.

We then obtain a series of ordinal numbers and a series of cardinal numbers. This latter is $\aleph_0, \aleph_1, \ldots, \aleph_n, \ldots$. We can prove the existence of all these numbers by admitting the axiom of infinity. But in my opinion we cannot prove, without a new axiom, the existence of $\aleph_\omega$, or of any cardinal number greater than all the numbers $\aleph_0, \aleph_1, \ldots, \aleph_n, \ldots$.[1]

By admitting the axiom of infinity, we have therefore proved the existence of two progressions of cardinal numbers, namely

$$\aleph_0, 2^{\aleph_0}, 2^{2^{\aleph_0}}, \ldots,$$
$$\aleph_0, \aleph_1, \aleph_2, \ldots.$$

25/491 If we do not admit the multiplicative axiom, there does not exist any proof that the terms of the first progression are either greater or smaller than the terms of the second progression (apart from the term $\aleph_0$). Cantor has hoped to be able to

[1] This is one of the places in this paper where the absence of Russell's type theory is apparent, for he eschewed types of transfinite order and thus could not define $\aleph_\omega$ anyway.

prove that $2^{\aleph_0}=\aleph_1$, but neither he nor anyone else has succeeded in proving such an equation.

There is another order of ideas which, starting from the axiom of infinity, proves the existence of dense series of various types. For example, let us arrange the finite integers after the following rule : Let us express the integers according to the dyadic scale, so that they have the form

$$\sum^{\alpha} 2^n,$$

that is, the sum of the numbers 2^n for each n which is a member of α. Then, given two numbers $\sum^{\alpha} 2^n$ and $\sum^{\beta} 2^n$, we shall put the first before the second if the smallest number which belongs to one of the two classes α, β without belonging to both classes belongs to α.

In such a case we obtain a series of numbers where the numbers which have the more zeros at the end will come later ; among the numbers which have the same number of zeros at the end, those which have the fewer 1's before the zeros will come later ; among the numbers which have the same number of zeros at the end and the same number of 1's before the zeros, those which have the more zeros before the 1's will come later, and so on. We can construct this series in the following manner : let us put 1 in the middle, 10 to the right, 11 to the left. Let us call *gaps* not only the spaces between two figures [chiffres], but also the spaces to the right and to the left of all the figures. Let us then fill these gaps, starting from the right, by inserting first the figure 0 without changing the order, then the figure 1 omitting the figures already obtained. In other words, abandoning the dyadic scale, place 1 on the first row ; below 1 put 2, 3 in the gaps to right and left ; below the second row, let us double the second row to fill the gaps to the right of 1 and add one to the numbers[1] already obtained for the third row to fill gaps to the left of 1.

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				3				1				2			
		7				5				6				4	
	15		11		13		9		14		10		12		8
31	23	27	19	29	21	25	17	30	22	26	18	28	20	24	16

To obtain the nth row, we start with 2^{n-1} at the extreme

[1] Russell put ' figures ' (' chiffres ') here by mistake.

right ; then we add 2^{n-2} to it ; then we add 2^{n-3} to the numbers already obtained,[1] while preserving the order ; then we add 2^{n-4} to the numbers already obtained, while preserving the order ; until we arrive at odd numbers, which fill the left-hand half of the row.

Continuing by this method, it is clear that we obtain a dense series composed of finite integers. Thus, on admitting the axiom of infinity, there are dense series having $\aleph_0$ terms. On taking the segments of such a series, we obtain a continuous series in the sense of Cantor, that is to say, a series of the type of the real numbers.[2] Therefore, on admitting the axiom of infinity, we prove the existence of the continuum. From there we can prove the existence (in the mathematical sense) of Euclidean and non-Euclidean spaces of all kinds.

From what we have just said, it is clear that the axiom of infinity suffices on its own to prove the majority of existence-theorems that we need in mathematics. However, there is another existence-axiom, namely the multiplicative axiom, which would be very useful if we could know that it is true. The usefulness of this axiom concerns more the theory of the transfinite than ordinary mathematics, whereas the axiom of infinity is necessary for the largest part of ordinary mathematics, for example for the infinitesimal calculus.

The multiplicative axiom has many equivalent forms. Zermelo, the first to state it explicitly, gives it a form which we can express in the following manner :

Given any class α, let κ be the class of all the non-empty classes which are part of α (including α itself). Then there exists a function f such that, if β is a member of κ, $f\beta$ is a member of β. In other words, there exists a rule according to which we can choose a representative term in each non-empty class contained in α.

27/493 In order to understand this axiom and its usefulness for multiplication, it is necessary to say a word about the theory of selections. Let κ be a class of classes which exclude each other mutually, then we call *selection of* κ a class which contains one

[1] Russell wrote '2^{n-1}' by mistake instead of '2^{n-3}', and also '2^{n-3}' instead of '2^{n-4}' in the next clause.

[2] For the construction effectively produces a rational order-type, and segments of rationals yield the type of the continuum via Russell's definition of irrationals. Peirce carried out a similar construction in a manuscript called ' Ordinals ' and apparently dated around 1905 (see his *Works*, vol. 4, 277–280), and the Youngs produced another in their *1906a*, 128. Note Russell's use of (Hausdorff's) ' dense ' rather than his own ' compact ' in this paragraph.

and only one term in each class which is a member of κ. We can call this term the *representative* of the class to which it belongs. For example, let us suppose that the Chamber of deputies is composed exclusively of inhabitants of the wards which they represent ; then the Chamber will be a selection of wards considered as classes of electors.

Now, when the number of members of κ is finite, we can make an arbitrary choice of the representatives of the classes, so that there always are selections. And it is obvious that the number of possible selections in this case is the product of the numbers of the classes[1] which are members of κ. We can use the class of selections to define this product, and we thus obtain a definition of the product which extends to the case where the number of factors is infinite. It is for this reason that the theory of selections is closely linked to the theory of multiplication.

First of all, it is necessary to make a little more precise the idea of selection. Whenever the number of members of κ can be infinite, it is impossible to create a selection by arbitrary choices. Therefore it is necessary to have a rule according to which the choices are effected. (For example, if the classes which are members of κ are series of which each has a first
28/494 term, we could always choose the first term.) Now a rule of choice is represented in logic by a one-many relation R such that, given that $\alpha \varepsilon \kappa$, there exists one and only one x which has the relation R to α, and this x is a member of α. That is to say, R will be a one-many relation of which the converse domain is κ and which implies ε, where ε is the relation of a term to a class of which it is a member. A relation such as R is called *selective relation* of κ ; we shall write it $\varepsilon'_{\Delta}\kappa$. In symbols,

$$\varepsilon'_{\Delta}\kappa = (1 \to \mathrm{Cls}) \cap \overleftarrow{\mathrm{C}}'\kappa \cap \mathrm{Rl}'\varepsilon \quad [\mathrm{Df.}]$$

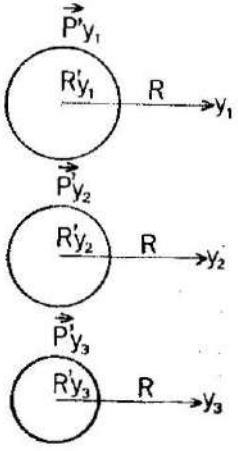
Now this notion can be generalised further, by putting

$$P'_{\Delta}\kappa = (1 \to \mathrm{Cls}) \cap \overleftarrow{\mathrm{C}}'\kappa \cap \mathrm{Rl}'P \quad \mathrm{Df.}$$

If we have $R \varepsilon P'_{\Delta}\kappa$, R will resemble the relation in the diagram [overleaf].[2]

[1] A rather clumsy phrase ; perhaps ' product of the cardinalities of the classes ' would be better. Russell used his phraseology later also.

[2] I have placed the diagram here instead of its rather inappropriate place in the paper three paragraphs above. Its purpose is to show that, given a y, there corresponds under R the object $R'y$, which belongs to the class $\vec{P}'y$ indicated by the circle. Compare this passage with *PM*, ✱80. Note that Russell's left apostrophes in notations were rendered as right apostrophes in this passage.



The general notion is useful, but for the moment we shall occupy ourselves principally with $\varepsilon'_{\Delta}\kappa$.

We remark first that the class of selections may define the arithmetical product only whenever the members of κ exclude each other, whereas $\varepsilon'_{\Delta}\kappa$ is not subject to this restriction. I write $\mathrm{Nc}'\alpha$ for *the cardinal number of* α, where α is a class (a collection);[1] I write $\Pi\mathrm{Nc}'\kappa$ for the product of the numbers of members of κ, where κ is a class of classes. Hence we can put

$$\Pi\mathrm{Nc}'\kappa = \mathrm{Nc}'\varepsilon'_{\Delta}\kappa \quad \text{Df.}$$

Then when κ is a finite class, $\Pi\mathrm{Nc}'\kappa$ will have the known properties of arithmetical products even if the members of κ are infinite classes. But if κ is an infinite class, we do not know that the class $\varepsilon'_{\Delta}\kappa$ is not empty if we do not admit the multiplicative axiom. Therefore it could happen that an arithmetical product is zero when none of its factors is zero. If we admit the multiplicative axiom, then this becomes impossible; in fact, the proposition: *a cardinal product can be zero only if one of its factors is zero*, is equivalent to the multiplicative axiom.

The multiplicative axiom can be expressed as follows: *Let κ be a class of non-empty classes; then there exists at least*
29/495 *one class μ which possesses one and only one term in each class which is a member of κ.*

[1] I have had to discriminate here between 'classe' and 'ensemble', giving the normal translation 'class' to 'classe'. Despite making the distinction here, Russell finished this sentence with 'classe des classes'!

Zermelo's axiom can be expressed as follows : *Let α be any class, and let κ be the class of all the non-empty classes contained in α ; then there exists at least one selective relation of κ.* In symbols

$$(\alpha) . \exists \mathrm{Is}'_{\Delta}\mathrm{Cl}\, \mathrm{ex}'\alpha.$$

It is easy to prove that these two axioms are equivalent.[1]

Thus, according to Zermelo's theorem, both [axioms] are equivalent to the proposition : *Any class can be well-ordered.* They are also equivalent to the proposition that, given a class κ of non-empty classes, there always exist selective relations of κ ; and to the proposition that, if P is any relation and κ a class contained in the converse domain of P, then $P'_{\Delta}\kappa$ is not empty.

The propositions which can only be proved by means of the multiplicative axiom are very numerous. Here are some of them :

1. Of two different cardinal numbers, one must be greater than the other.

2. The cardinal numbers which are increased by the addition of unity are the same as the inductive numbers, that is, the numbers which we call *natural.* This proposition identifies the two definitions of the infinite. We could say that a class is infinite whenever it contains a part that we could put in one-one relation with the entire class ; according to this definition, a number is infinite whenever it is not increased by the addition of unity. Or else we can say that we call *finite whole number* any number which obeys complete induction starting from zero. That is to say : let us call *recurrent property* any property which holds for $n+1$ given that it holds for n, and let us call *inductive property* any recurrent property which holds for zero. Then we shall call *inductive number* any number which possesses all inductive properties, that is to say, any number for which proofs by means of complete induction are valid. It is easy to see that the inductive
30/496 numbers are the same as the natural numbers 0, 1, 2, . . ., 100, . . ., 1000, . . ., and that (admitting the axiom of infinity) there are numbers which are not inductive numbers, for example, the number of inductive numbers. The so-called *principle* of complete induction becomes therefore a definition, namely, the definition of inductive numbers. Then we can say that the infinite numbers are the non-inductive numbers. Cantor always presumes that the non-inductive numbers are

[1] In a footnote Russell now cited *PM*, ✳88 ; more precisely, see ✳88.33.

the same as the numbers which are not increased by the addition of unity; but to prove this we need the multiplicative axiom, not in its full generality, it is true, but only for the products of $\aleph_0$ factors.

3. We define the product of *two* factors in the following manner: let α and β be any two classes; then the product of α and β will be the class of couples (x, y) of which $x \varepsilon \alpha$ and $y \varepsilon \beta$ (distinguishing x, y from y, x). Then we prove the majority of the known properties of the product of two factors without the multiplicative axiom. However, if we want to establish the relationship between addition and multiplication, we need the axiom; that is, we need it to prove that the sum of μ classes, of which each has ν terms, possesses $\mu \times \nu$ terms. It is the same for the relationship between multiplication and exponentiation.

4. It follows from what we have just said that without the multiplicative axiom we cannot prove that the sum of $\aleph_0$ classes, each having $\aleph_0$ members, possesses $\aleph_0 \times \aleph_0$ members. We know that $\aleph_0 \times \aleph_0 = \aleph_0$, and it is usual to deduce from it that the sum of $\aleph_0$ classes each having $\aleph_0$ members has $\aleph_0$ members. However, this deduction is valid [bonne] only if we admit the multiplicative axiom.

5. By means of the deduction of which we have just spoken, we are accustomed to proving that the limit [ordinal] of a progression of ordinal numbers of the second kind (that is to say, formed by series of which the cardinal number is $\aleph_0$) is itself of the second kind. This is also valid only if we admit the multiplicative axiom; thus a very large part of the theory of transfinite ordinal numbers becomes doubtful. It is the same with the theorem that in any series a term cannot at the same time be the limit of a series of type ω and of a series of
31/497 type ω_1. It follows that practically all of the beautiful work of Hausdorff, *Untersuchungen über Ordnungstypen* [*1906a* and *1907a*], depends on the multiplicative axiom.

6. The multiplicative axiom is necessary to prove that the sum of μ classes each having ν terms has the same number of terms as the sum of ν classes each having μ terms, except in the case where μ and ν are both finite. To take the most simple case, let us presume a progression of couples

$$x_1, y_1, x_2, y_2, x_3, y_3, \ldots, x_\nu, y_\nu, \ldots,^1$$

[1] A rather unfortunate use of 'ν' as a suffix variable, for it has no necessary connection with the 'ν' of the preceding lines.

and let us suppose that we have to prove that we can arrange the same terms in a couple of progressions. With the notation that we have just used, this is done by taking the series

$$x_1, x_2, \ldots, x_\nu, \ldots, y_1, y_2, \ldots, y_\nu, \ldots .$$

But, in adopting the notation x_ν, y_ν for the ν-th couple, we have introduced the hypothesis that an order is given for each couple such that we can distinguish a first from a second member of the couple. If we do not make this hypothesis, it will be impossible to give a rule according to which we choose simultaneously a term in each couple. Thus we do not know how to arrange the terms in two progressions.

What we have just said will become more clear by taking an example. There once was a millionaire who possessed $\aleph_0$ pairs of boots. Can we prove that that number of boots that he possessed was an even number ? Yes, for we can put all the left boots in one class, and all the right boots in the other class. But if this millionaire had the eccentricity to have the two boots the same, so that there was not a right boot and a left boot in each pair, then it becomes impossible to effect the division of the class into two equal parts. Therefore it is impossible to show that the number of his boots was an even number, or that there were $\aleph_0$ boots, in spite of the fact that we have

$$2 \times \aleph_0 = \aleph_0 \times 2 = \aleph_0.$$

Mathematicians used the multiplicative axiom un-
32/498 consciously, not only in set theory but also in ordinary Mathematics, until Zermelo stated it explicitly as an axiom.[1] I used it formerly myself without knowing it ; but I made the discovery in 1904 that there was here an independent axiom. Many mathematicians, like Zermelo himself, assert that this axiom is as evident as the others, and that we can affirm it without hestitation. Others say that there is no reason to believe that the axiom is true. Peano,[2] after having proved the independence of the axiom, devotes only the following remark to the discussion of its truth :

> ' Now must we believe that the proposition is true, or that it is false ? Our opinion is unimportant '.[3]

[1] In a footnote Russell now cited Zermelo *1904a*.

[2] In a footnote Russell now cited Peano *1906a*, 145–148 (also in *Works*, vol. 1, 346–349 ; *1973a*, 207–210).

[3] Russell now cited in his text p. 148 of Peano *1906a* (*Works*, vol. 1, 349 ; *1973a*, 210). Peano's ' proof ' of independence is merely an intuitive suggestion that infinite selections are independent of the axioms and rules of quantification theory ; it is no anticipation of Fraenkel *1922a*.

Peano says in the same article that the question of evidence is a psychological question, which does not concern logic. However, logic depends on the axioms of logic, and we admit these axioms because they are evident; at least, the evidence either of the axioms or else of their consequences is the sole cause that makes us admit the axioms. Therefore it is permitted to ask if the multiplicative axiom is true, in spite of the fact that it is not deduced from the other axioms. It is necessary to recognise that there is not much to say about this question; but it is possible to present some remarks which tend to give rise to doubt with regard to the axiom.

As for the claimed evidence for this axiom, it seems to me that the imagination always presents to us a finite number of classes, even when we want to speak about an infinite number. Now we do not need an axiom in the case of finite numbers, since the possibility of selections is proved easily. Therefore the apparent evidence for the axiom tends to disappear under the influence of reflection. Further, when we speak of an infinite class, it is necessary that it be given by means of a property which all the members of the class possess and which others do not possess: it is impossible that the class be given
33/499 by enumeration. Therefore the multiplicative axiom must affirm that when we have a collection of classes[1] there always exists some property that one and only one term in each class which belongs to the collection possesses. Now in my opinion this is not at all obvious. I find myself drawn to the conclusion that the axiom ceases to be evident from the moment that we realise what it signifies.

It is also necessary to say that Zermelo's theorem, namely that if the axiom is true then any class can be well-ordered, gives grounds for believing that the axiom is false; for it is hardly believable that every class can be well-ordered. Many clever mathematicians have endeavoured to find a well-ordered series composed of the real numbers, but no-one has succeeded in finding such a series. Such arguments do not have much force, but it is necessary to accord to them a certain value.

It is possible that we shall find later a *reductio ad absurdum*[2] which will show that the axiom is false. But at the moment it seems to me that it is only doubtful. It is possible that it is

[1] Once again I translate 'ensemble' as 'collection' and 'classe' as 'class'. In line with his axiom of infinity, Russell uses the word 'infinite' here in its non-inductive rather than its reflexive sense.

[2] Russell used the French translation 'réduction à l'absurde'.

true, but the evidence is lacking for this and its consequences are surprising. In these circumstances it seems to me that we shall do well to abstain from using it, except in arguments which give hope of leading to an absurdity and thus to settling in the negative the question of the truth of the axiom.

There exists an important difference between the multiplicative axiom and the axiom of infinity from the point of view of the theory of knowledge. The multiplicative axiom has the form and character of the axioms of logic : we do not know how to prove it by means of empirical data. The considerations which bear on the truth or falsity of the multiplicative axiom are considerations of logic, *a priori* considerations. By contrast, the axiom of infinity is purely empirical. Whatever be ν provided that ν is a finite or transfinite cardinal number, it is possible *a priori* that ν is the number of individuals in the universe. But according to empirical evidence, according to the divisibility of physical objects, it seems artificial to suppose that there is a finite number of individuals in the universe. It does not seem to me that empirical data suffice to *prove* that the number of individuals is not finite, but they suffice to show that the finitist hypothesis is much more difficult and less simple than the other, and the logic of
34/500 the infinite shows that the finitist hypothesis is not at all preferable *a priori*. I conclude from it that, for the reasons which usually decide scientific hypotheses, it is better to presume that the number of individuals is infinite while still bearing in mind that this hypothesis could be false, although the perceptible facts are such that we could never know that it is false.

To resume, we have seen that, in spite of the fact that all pure Mathematics can be represented as a prolongation of logic, the existence-theorems that we want to prove often need two axioms which cannot be proved by means of the principles of logic, although we can express them in logical terms. These two axioms are the axiom of infinity and Zermelo's axiom (in other words, the multiplicative axiom).

The axiom of infinity is necessary and sufficient to prove that there exist progressions, that is, series of the form $x_1, x_2, x_3, \ldots, x_\nu, \ldots$ to infinity. It is necessary and sufficient to prove the existence of the cardinal numbers $\aleph_0, \aleph_1, \ldots, \aleph_\nu$ (where ν is finite), $2^{\aleph_0}, 2^{2^{\aleph_0}}$, etc. ; of the ordinal numbers $\omega, \omega_1, \omega_2, \ldots, \omega_\nu$ (where ν is finite) as well as all their intermediate [ordinals] ; of the order-types η and θ (that is, the

types of rational numbers and of real numbers); as well as a mass of other order-types. The multiplicative axiom, which can be expressed in many equivalent forms, is necessary and sufficient to show that an arithmetical product, of which the number of factors is infinite, can be zero only if one of the factors is zero, as well as to show that every class can be well-ordered. The axiom suffices to prove many theorems of capital importance in the theory of the transfinite, that we do not know how to prove otherwise; it is sufficient, for example, for the identification of the two definitions of the infinite. But in my opinion there is no reason to believe that the multiplicative axiom is true, whereas for empirical reasons it seems probable that the axiom of infinity is realised in the actual world. For my part I find that we do well at present to use either of the two axioms only as an explicit hypothesis.

The axiom of infinity is only necessary because of the theory of logical types, which serves to resolve the paradoxes. There-
35/501 fore it is possible that the axiom of infinity will cease to be necessary on modifying the theory of types.[1] As for the multiplicative axiom, it remains possible that on searching among its consequences we shall end up with a contradiction, which will prove that the axiom is false. But for the moment the two axioms remain doubtful; it is possible that both are true, and it is possible that one is true and the other false. The only thing that we know to be impossible is that both are false; for if there is not an infinite class, then selections must always be possible, so that the multiplicative axiom must be true.[2]

It is to be hoped that such a small result as this will soon be enlarged by the work of mathematicians.

27. *Philip Jourdain: some contributions to* The Granta

Since 1889 *The Granta* has been the humorous magazine of Cambridge University, and especially during the period 1904–1909 its pages were graced by the barbed witticisms of Philip Jourdain, in the form of fables, tales and poems. Jourdain signed his contributions either with

[1] We recall that Russell (reluctantly) admitted the axiom of infinity into his system when he committed himself to the type theory of *Principia Mathematica* (see pp. 105–106 above). But this statement suggests that he felt that the axiom might be dispensable in a modified type theory. On such points see Bernays *1966a*, and Quine *1969a*, 279–299 *passim*.

[2] Compare *PM*, ✳120.492, ✳120.64.

his initials (' P.E.B.J.' or just ' P.J.') or with identifying Russellian descriptive phrases such as ' By the author of " Topsy-Turvy Fairy Tales " '. Unfortunately it was often the custom in *The Granta* to publish anonymously, and apparently no means is now available to identify the authors. I strongly suspect that some anonymous pieces have Jourdain's authorship, for they certainly show his style ; but with the lack of positive identification I have regretfully had to let them pass. The contributions which are positively identifiable include not only the ' Topsy-Turvy Fairy Tales ' but also a splendid series on ' Some unconscious humorists of the nineteenth century ', and a scattering of verse. With permission of the current management of the magazine, I have chosen four items which are closely related to his discussions with Russell and which have been mentioned in earlier sections. The first two, *1905d*, and *1906i*, 124 (*1913i*, 311–312) concern the forms of the liar and visiting card paradoxes which I suggested on page 50 above might be due to Berry. The second two, *1907b*, are the first manifestations of ' The philosophy of Mr. B*rtr*nd R*ss*ll '. The footnotes are my own.

The Wise Man's Problem.

One day a wise man was walking in a wood, meditating on some of the graver questions of Life and Death, when he suddenly came upon another Wise Man, who was sitting with his watch in his hand, saying the most absurd things, such as :

' Everything comes to him who waits '.

' All's well that ends well '.

Then he looked up and said : ' All my remarks between the times of two o'clock and half-past two are lies '. The first Wise Man saw that the watch was at one minute to the half-hour.

The second Wise Man then uttered that hoary old falsehood : ' Wolves are found in every fold ', and shut his watch with a snap.

' Now then ', said he, ' was my remark of a minute ago true or false ? I may add that all my remarks before you came, from two o'clock onwards, were false '.

' Then that remark *was* true ', said the other.

' Indeed ! ' snapped the story-telling philosopher, ' if that remark *was* true, since it was that all my remarks made in a certain period of time were false, and *it* was made in that period, *it* must have been false too ! '

' Then it must have been false ', meekly replied the unfortunate Wise Man.

' Indeed ! ' roared the other, ' pray tell me how it *could* be false, since, if it were, at least one of my remarks in that period of time

must have been true, and, therefore, since all the others were certainly false, that remark must have been true ! '

So the poor, puzzled, Wise Man (the one I began about) sat down, and buried his face in his hands to think it all out. The other one went off, relieved at having transferred his puzzle to someone else.

* * * * * *

Two hours afterwards the Wise Man's Wife (I hear some *blasé* Baby, who has done me the honour to read this Tale, say that he could not have been so wise after all, if he married ; but just wait till the end of the story) came to fetch him in to tea. She found him still puzzling over the remark that was both true and false, so he had to explain things to her.

' Didn't you say the man told you he was telling lies ? ' asked she.

' Yes ', said he, ' but— '

' But, indeed ! ' said she scornfully, ' you silly fellow to take any notice of what he said after *that* ! '

He opened his mouth to speak, but thought better of it, and only opened it again at the tea-table—for another purpose.

* * * * * *

Now, what do you think of his wisdom in marrying such a treasure ?

The Philosopher's Revenge.

Once upon a time two philosophers (whom I will call A and B) quarrelled. I quite forget what the quarrel was about, but it was very trivial compared with the dreadful revenge taken by A.

First of all, B, deeply insulted, tried to express his contempt for A by sending him *Emerson's Essays*. A retaliated with some plays by Mr. G. Bernard Shaw. Then both lost their tempers, and bad English was bandied about in the shape of works by Miss Marie Corelli.[1] Then A struck the final blow.

One dark, windy night A crept up to B's house and put a bit of cardboard into B's letter-box. On it were some sentences which A had cunningly contrived to give acute pain to the philosophic mind.

* * * * *

[1] Miss Marie Corelli was a popular light novelist who tended to sentimentalise on Christianity. The latter property qualified her for Jourdain as an unconscious humorist of the nineteenth century, as he explained in *1906g*.

The next morning B found the card, and read :—

The statement on the other side of this card is false.

' Well, suppose that is true ', thought B, he turned over the card and read :—

The statement on the other side of this card is false.

' *This* is false, then, so the statement on the other side is true. That confirms my hypothesis ! ' So he borrowed a pencil in a gold case (which he forgot to return) from a neighbour, and marked the first side he had looked at ' true '. The he went into breakfast quite happily, and showed the card, with a chuckle, to his wife.

' Why have you written " True " on this side ? ' asked she ; ' the other side says it is false'.

' Why, don't you see, it is true because the statement on the other side is false'.

' Stuff ', replied she, ' there is no reason why it shouldn't be false, and the statement on the other side true ! '

As sometimes happens with women, her wild, unpremeditated shot hit the mark. So poor B had no retort ready, and had to content himself with grumbling at the hardboiledness of his egg (which was, as every housewife knows, just right).

* * * * *

In the course of the morning B had an inspiration. ' Every woman ', said he, repeating one of those silly generalities about women indulged in by people like Max O'Rell or Dr. Emil Reich,[1] ' assumes that a statement is false unless it proves to be true,— and then, if a true woman, she still thinks it false '. So he wrote another card :—

The statement on the other side of this card is true.

' She will think this false, so that when she turns it over, and reads—

The statement on the other side of this card is false,—

she will have to think the first statement *true*. Won't that puzzle her ! Perhaps she will toast me a muffin for tea ', he added with true insight.

* * * * *

At the luncheon table B's wife glanced at the card, sniffed :— ' True, is it ? ', and turned the card over.

' I *knew* it was false ', added she then. ' Do you mean to say *that's* what you have been doing all the morning ? '

' No ', said he untruthfully, ' I— '

[1] O'Rell was a novelist, and Reich an historical essayist.

'You had much better have been mowing the lawn', replied she.

* * * * *

So, as a result of A's wicked deed, B not only got no muffin for tea, but, ever afterwards, his wife held him and his logic in even greater contempt than she had before.

But the lesson was not wholly wasted. B gradually came to perceive the great truth that falsehood, in logic as in other things, though a good servant, is a bad master.

Essays on Logic
*By the Hon. B*rtr*nd R*ss*ll*

I. The Law of Identity

It will probably be admitted at once that the statement '*a* is *a*' is a truism, and one would hardly expect to meet such things outside of a logic book. And yet it is a remarkable fact that identities are frequently used in common life by people who seem to imagine that they can draw important conclusions respecting conduct or matters of fact from them. I have heard of a man who gained the reputation of being a fatalist by the repeated enunciation of the identity, 'Whatever will be, will be', and the Italian equivalent of this, 'Che sarà, sarà', makes up an appreciable part of Mr. Robert Hichens' latest novel. Further, the identity 'Life is Life' has not only often been accepted as an explanation for a particular way of living, but has even been considered by 'Zack' to be an appropriate title for one of her novels,[1] while 'Business is Business' is frequently considered to provide an excuse for dishonesty in trading, for which purpose it is plainly inadequate. Another example is given by a poem of Mr. Kipling's. He uses the truisms 'East is East' and 'West is West', to prepare the way for the fallacy 'and never the twain shall meet'. I have said 'to pave the way', although it is plainly meant to be an implication; otherwise it is difficult to see why Mr. Kipling should have used the word 'and', which is the wrong one to denote an implication. The conclusion, now, is palpably false; for since the world is round—as most of us have been convinced long ago, by arguments which strike every intelligent child as ludicrously inadequate—what is called the 'west' does in fact border on the 'east'. Even if we are to take the statement metaphorically, it is still untrue, as the Japanese nation shows.

[1] 'Zack' was the pseudonym of Gwendoline Keats.

II. The Word 'The'

The word 'the' implies existence and uniqueness; '*The* son of So-and-So' implies that So-and-So *has* one, and *only* one, son. Thus, people who talk of 'The Oxford Movement imply that Oxford only moved once, and the middle-class persons who way that 'A is quite the gentleman' imply both the doubtful proposition that there is only one gentleman in the world and the indubitably false proposition that it is that man.

In a certain Children's Hymn Book, one reads:

The river vast and small.

Few would deny that there is not more than one such river, but unfortunately it is doubtful whether there is such a river at all. The case is exactly the same with the ontological proof of the existence of the most perfect being.[1]

28. *Jourdain's copy of* Principia Mathematica

We noted on page 136 above that Jourdain received a complimentary copy of the first volume of *Principia Mathematica* and discussed a few points with Russell. After reading this exchange I was anxious to see the copy, but I discovered that Jourdain's library had been widely dispersed after his death and that any kind of systematic search would probably be fruitless. Then Dr. J. M. Rollett drew my attention to the fact that in 1967 a London bookseller, H. K. Elliott, had advertised for sale Jourdain's set of all three volumes of *Principia Mathematica*, and had described it as containing 'numerous interesting manuscript notes throughout'. Unfortunately, by the time of this discovery Elliott had died and there was no surviving note of the purchaser of the volumes; but their apparent significance was now increased, and I published a request *1974d* for information about them. To my great surprise and delight I received a letter from the son of their present owner (Dr. H. F. Norman of San Francisco), and later received photocopies of the pages on which Jourdain made substantial annotations (as opposed to mere underlinings or similar marks). With Dr. Norman's kind permission, I am able to devote this section to an account of the most interesting of these annotations. They fall naturally into three groups, and I have numbered them consecutively throughout.

Apparently all the important markings come from the first volume. Since the first edition of *Principia Mathematica* is now rather rare, I have given also the corresponding locations in the second (1925) edition

[1] Compare pp. 45 and 126 above.

of the volume, which is much more accessible. The manner of citation is exemplified by :

58, 6u (56, 5d),

which states that the sixth line up (including lines of footnotes) from the bottom of page 58 of the first edition contains text which appeared on the fifth line down (excluding running-heads) from the top of page 56 of the second edition. All the editorial practices are still in operation, as is the principle of minimisation of editorial commentary.

The inside cover of the volume carries Jourdain's bookplate, which illustrates Achilles chasing the tortoise (one of his favourite problems). There is also an authors' compliments slip on which he has put the date 7 December 1910—presumably the date of receipt. Then four comments are written on a following blank page.

1. ' Does Frege's theory of *Ordnungen* anticipate the theory of types ? ' Jourdain must have been thinking of Frege's stipulation that a function whose arguments are objects (of the first level) cannot take itself as an argument, but may be an argument for a function of the second level. Jourdain mentioned Frege's theory in his *1912a*, 265–266, but did not pursue the question that he had raised here. The similarities between Frege's and Russell's theories are obvious, but Frege's care over use-mention distinctions and Russell's awareness of the paradoxes make the differences thereafter substantial. For a significant discussion of Frege's theory, see Sluga *1962a*.

2. ' Why is Frege's distinction of *Sinn* and *Bedeutung* rejected ? (cf. p. 70, last par.) '. In this paragraph (p. 67, 11u–5u of the second edition) Russell summarised his theory of descriptions and concluded that ' " the author of Waverley " means nothing '. Frege was not cited, but in his ' On denoting ' Russell argued against Frege's assignment of such phrases to the empty class (Russell *1905a*, 483–585 (*1906a*, 45–48 ; *1973a*, 108–111)). As with their theories of types, fundamental differences of viewpoint are involved here, concerning which reference is recalled to the secondary literature cited on page 71 above.

3. ' Principle of contradiction very subsidiary (p. 116) '. This remark relates to p. 116, 7d (111, 3d), where Russell stated that ' The law of contradiction, " $\vdash . \sim(p . \sim p)$ ", is proved in this number ; but in spite of its fame we have found few occasions for its use '. As we noted especially on pages 112–113 above, Russell conflated theory with meta-theory, and so associated the law of contradiction with ' $\sim(p . \sim p)$ ' of his object theory. In similar vein Jourdain added to the footnote of the first paragraph of ✳1 a little-known reference in Russell (*1903b*, 187) on the difficulty of discussing the foundations of

logic. Once again he showed both his awareness of this problem in Russell and the thoroughness of his acquaintance with Russell's writings.

4. ' Forsaking of analogy in $\alpha \subset \beta$ on p. 28 '. This may be a reference to p. 28, 3u (28, 2d) where connectives between propositions and between classes were correlated. It was explained that identity rather than equivalence between propositions must be correlated with class equality. We recall that ' $\subset$ ' denotes improper inclusion in Russell's system, and wonder how he would define identity between propositions. Perhaps something like

$$p = q \,.\, = \,.\, (\phi) \,.\, (\phi p \supset \phi q) \quad \text{Df ?}, \tag{28.1}$$

where ' ϕ ' ranges over logical combinations of propositions, would have to do, though the meaning of such a definition is obscure.

P. xv of the volume contains a printed list of errata. Jourdain conveyed them all into the text, but also wrote down several other comments, which form our next group of annotations. A few closely related comments written in the main text are also dealt with.

5. Three points concern the definitions of V and of $\vec{R}`x$, and the use of ' $\breve{R}$ ' for the converse relation of R, which we saw Russell discuss on 2 January 1911 (page 136 above) soon after Jourdain had received the volume. In fact, Jourdain has made a note of Russell's letter, and it is not necessary to repeat these points here. But there is a relevant comment later in the volume concerning the definition of V :

$$V = \hat{x}(x = x) \quad \text{Df} \tag{28.2}$$

(30, 16u (29, 16u)). It reads : ' Here and on p. 229, ought not the definition to be made *homogeneous* ? For we define a *constant* V as $\hat{x}\phi x$, so V is apparently a function of ϕ '. Perhaps Jourdain had in mind Russell's remark at the second reference mentioned (229, 9d (216, 9d)) that ' Any property possessed by everything would do as well as " $x = x$ " '. But such a property would be equivalent with ' $x = x$ ', and Russell's reply of January 1911 seems adequate. Jourdain's use of ' homogeneous ' is especially strange, since (28.2) is restricted to a type and is typically ambiguous.

6. ' p. 618. Ought not Dedekind to be referred to in connexion with Zermelo's proof or of taking an $\aleph_0$ from any infinite class ? ' This question refers to p. 618, 4u (589, 10u), where Russell described Zermelo's proof of the Schröder–Bernstein theorem. Perhaps Jourdain had in mind Dedekind's use of similar techniques in his *1888a* (*1887a* was not then published).

7. ' p. 32. Double use of the word " existence " (for $\exists$ and E) confusing. I think mathematicians usually mean E by " exists ", and

this was why Poincaré disagreed with Couturat'. Jourdain also referred this statement to other places in the volume, and we have seen it often in the correspondence (especially on pages 71–73 above). By way of emphasis, at one of the other places which he cited (69, 14u (66, 17u)) he wrote in the margin ' rather : have Being (?) ', which was one of his (and Russell's) terms to describe the referent of an " existent " descriptive phrase. The ' disagreement ' mentioned by Jourdain may be found in Poincaré *1905a* and Couturat *1906a*.

8. Jourdain regarded as superfluous the statement on p. 17, 17d–18d (16, 8u–7u) that ' It follows from the meaning of " $(\exists x) . \phi x$ " that the x in this expression is also an apparent variable '. Certainly the sentence seems to add nothing to the explanation of apparent (bound) variables in the paragraph to which it belongs, but it appeared unchanged in the second edition.

9. Jourdain queried Russell's statement on p. 36, 3u–1u (35, 19u–17u) that ' Relations which are transitive and symmetrical are an important class : they may be regarded as consisting in the possession of some common property '. In the text Jourdain inserted ' and reflexive (?) ' to the conditions in order that the description of equivalence relations would be complete. The reprinted text, however, was not changed. Also on this page Jourdain corrected Russell's statement of two of the laws obeyed by the product of two relations, replacing ' $Q|R$ ' with ' $R|P$ ' at p. 36, 7d and 10d so that the laws read as they do in ✳34.26 and ✳34.24 respectively. In the second edition the first of these corrections was made at p. 34, 7u but the second left in error at 4u.

The final group of substantive comments comes solely from the main text. I have collected together those of like theme.

10. Jourdain compared the structure of the three introductory chapters with the papers that Russell had published previously, especially ' Mathematical logic as based on the theory of types ' (*1908a*) and ' La théorie des types logiques ' (*1910c*). The only point that seems worthy of notice occurs in a paragraph on the definition of implication. After asserting that the ' essential property ' of implication was that ' What is implied by a true proposition is true ', Russell wrote : ' But this property by no means determines whether anything, and if so what, is implied by a false proposition ' (98, 14d (94, 11d)). Jourdain noted that the corresponding sentence in Russell *1906a*, 161 continued : ' or by something which is not a proposition at all '. Jourdain recalled again Russell's letter of 2 January 1911 (page 136 above), where Russell had made a similar statement about implication.

11. Jourdain marked several places early on in the volume which

illustrated Russell's tendency to identify a symbol with the object symbolised. To both of Russell's assertions that ' a variable preserves a recognisable identity in various occurrences throughout the same context ' and that ' variables will be denoted by single letters ' (5, 3d and 13d (4, 2u and 5, 9d)) Jourdain wrote ' symbol ! '. To the beginning of Russell's account of truth-values on p. 8, 7d (7, 2u) he has put ' inconsistency, truth-value of a proposition and a symbol ' (if my conjectured reading is correct). At p. 9, 3d–12d (8, 8u–9, 2d), where Russell discussed assertion of and inference between propositions, Jourdain added : ' is a proposition a sign ? (before this, a proposition did not seem to be a sign) '. At p. 9, 3u (9, 14u) he supplemented Russell's explanation of Peano's dot convention in formulae using ' signs ' of connectives with the comment ' signs should be in " " '. A little further on Russell described the use of the convention with implication (10, 19u–14u (10, 13d–19d)), and Jourdain justly commented : ' A *sign* imply ? ' He also queried Russell's presentation of the axiom of identification of real variables (13, 3u (13, 21u)) because ' variable a sign '. Finally, when Russell began to explain the notion of a propositional function (15, 12d–14d (14, 9u–7u)) Jourdain wrote : ' a propositional function is a sign '.

12. A couple of interesting remarks related to the vicious circle principle turn up on p. 40. At 14d (38, 7d), in the middle of Russell's presentation of the principle, Jourdain noted : ' Compare Cantor's (and my) " limitation of size theory " '. Further down the paragraph Russell used his principle to claim that a sceptic cannot assert that he is ignorant of his own ignorance (40, 24d (38, 16d)), and Jourdain commented : ' Compare refutation of Spencer, and of the people who assert nothing '. Presumably he was thinking of Russell's attacks on evolutionary epistemologists such as Spencer (see especially Russell *1914e*). Perhaps he even recalled Russell's exclamation of 20 October 1908 (page 126 above) that ' Anything evolutionary always rouses me to fury '.

13. Two more comments close together relate to the paradoxes. Jourdain noticed that in the course of stating Burali-Forti's paradox Russell made the Jourdainian statement that ' the series of all ordinals (in order of magnitude) is well-ordered ' (63, 12u (60, 3u)). Jourdain contrasted this remark with its rejection in *The Principles*, art. 300. He also noted that in footnote ‡ of the next page Russell described Hobson's solution in *1906a* of the paradox of the least indefinable ordinal as being ' in outline in agreement with the solution adopted here ', whereas in *1908a*, 223 (*1956a*, 60 ; van Heijenoort *1967a*, 153) Russell had stated that the solution ' does not seem to me adequate '. However, Russell also expressed cautionary views of Hobson's solution in this footnote.

14. On p. 98, in the course of Russell's exposition of the propositional calculus in ✳1, Jourdain made an extensive note, which continued on to the back end-papers, about the treatment of the calculus by some of Russell's successors :

> Sheffer (*1913a*) defined all four truth-functions (two are here indefinable) in terms of a single function $p\Delta q$ (Nicod's $p|q$) :
>
> $$\begin{aligned} \sim p \;.\; &= \;.\; p\Delta p && \text{[Df]} \\ p \vee q \;.\; &= \;.\; \sim p \Delta \sim q && \text{[Df]} \\ p \supset q \;.\; &= \;.\; p \Delta \sim q && \text{[Df]} \\ p \;.\; q \;.\; &= \;.\; \sim(p \Delta q) && \text{[Df.]} \end{aligned}$$
>
> The reduction leads to a reduction to three of the Pps.

After citing Van Horn *1917a* and Nicod *1917a* (but not Stamm *1911a*), he continued :

> Nicod : to read $p|q$ as meaning the same as $\sim p \vee \sim q$ (as do Sheffer and Van Horn) is simpler (for definition of $p \supset q$) than treating it as $\sim p \;.\; \sim q$, and then corresponds to W. E. Johnson's ' Disjunctive Relation '.

The citation of Van Horn's and Nicod's papers of 1917 indicates the lateness of this addition. Nicod's reduction of the axiom system to a single axiom is well-known ; Van Horn's contemporary and independent work also used the Sheffer stroke, but achieved rather less scope with three axioms. Johnson's ' disjunctive relation ' is his identification of the disjunction of two propositions with $\sim p \;.\; \vee \;.\; \sim q$; see, for example, his *1892a*, 19.

15. In addition to the latter part of the passage above, the back end-papers also contain a bibliography of Whitehead's and Russell's own papers on logic, including a summary of the contents of Whitehead *1902a*.

This is the end of Jourdain's critical notes. What a pity that he did not write more.

Bibliography of published works

Most publications are cited by the name(s) of the author(s) followed by a dating code, which is the year of publication and a sufficial letter. Thus we have, for example, ' Russell *1908a* '. The letter is added even if the author published only one work in the year in question, in order to avoid any possible confusion with the year itself. For ' in *1908* Russell proved that . . . ' might be taken to refer to the year 1908 even though the dating code is italicised, whereas with ' in *1908a* Russell proved that . . . ' no possibility of misreading can arise—or if it does, then it cannot be excused.

There are a few exceptions or modifications to this system. Collected or substantial selections of works of an author are cited by a suitable catchword (such as ' *Works* ' or ' *Papers* ') to indicate their special character. The date(s) of publication have not been added, however, since the occasion of their publication did not bear upon the historical developments concerned. Further, book-length anthologies of a small part of an author's output (especially some of Russell's books, such as *Mysticism and Logic*) have been given a dating code as usual.

The other exception to the system is *Principia Mathematica*, which is cited in footnotes simply as ' *PM* ' without mention of its authors or of its years of publication. Wherever possible the article numbers are cited, but when it is necessary to cite a page number then the number is that of the *second* edition of 1925–1927 and not of the first edition of 1910–1913 (which is now rare). For some reason the Cambridge University Press reset some of the text of the first two volumes of the work and resited the rest (incorporating a very few references to the new introduction and adding the list of definitions), so that the page numbers of a particular passage are different. The third volume was reprinted without change (apart from the correction of printing errors).

A collected edition is given first in the list of an author's works, and is followed by the works cited by dating code, in chronological order. It is not easy to establish the exact chronology of papers published by an author in different journals at about the same time; hopefully the order given here, as represented by the alphabetical order of the sufficial letters to the same year date, is accurate in coarse listing while possibly inaccurate in fine detail.

A number of works have been reprinted in new or collected editions, in anthologies of the author or some other editor, in translations, or in different English and American editions. In order to minimise the citation of all of these various manifestations, specific passages are cited by chapter, article, or even theorem or axiom numbers. However, page numbers are sometimes unavoidable, and then some of the principal reappearances are also cited in brackets; for example:

Russell *1905a*, 489 (*1956a*, 52; *1973a*, 114).

It is a great pity that editors and translators have not in general learnt the need to indicate the original pagination of works reprinted or translated. For among other reasons, other literature published before the appearance of the new edition can only cite the original pagination, thus leaving stranded the reader who has available only such an edition with that pagination omitted. An edition which is equipped with the original pagination does not require special citation, and has not received it in the text. It *is* listed, however, in the bibliography.

In the case of Russell, especially, there have been so many reprintings and translations that it stretches the capacity of even a professional bibliographer to keep abreast of the literature, and far exceeds the abilities of a mere historian. Translations into English of works originally published in other languages have been listed, usually after their main entry and prefaced by the letter 'E', but translations of English works into other languages have been omitted. Hopefully the interested reader will be able to find one of the sources listed.

To save space in the bibliographical entries, the number(s) of particular edition(s) of a book are indicated by the appropriate subscript numbers, *photographic* reprints are indicated by ' = ', and titles of journals have been abbreviated in a manner similar to conventions used in international catalogues of serials. Any other editorial additions or comments are enclosed in square brackets.

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[1] Russell insisted before his death that 'Archive' be changed to 'Archives'.

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1908b. ' Untersuchungen über die Grundlagen der Mengenlehre. I [and only] ', *Math. Ann.*, 65(1908), 261–281. E : van Heijenoort *1967a*, 199–215

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Glossary of principal notations

This glossary contains the principal notations used both in the correspondence and in the commentaries. More specialised notations are specified either in the text or in footnotes. Note that several of the notations are *not* those finally adopted in *Principia Mathematica*, and that the correspondence describes some changes in notation.

Many of the notations in the calculi of classes and relations, and in cardinal and ordinal arithmetic, may refer to some property at one place in the correspondence and to the corresponding defined class in another place. The qualifying phase ' class of ' is placed in brackets at the front of the descriptions of these notations, and the subsequent plural is to be replaced by the corresponding singular. Thus, for example, the description of ' $1\rightarrow 1$ ' as ' (class of) one–one relations ' means that ' $1\rightarrow 1$ ' may be used in the correspondence to say of R either that it belongs to the class of one–one relations or that it is (that is, has the property of being) a one–one relation. The context makes clear the sense used, though the mixture of senses (due to Peano) is unfortunate !

Although several descriptions of the notations are formulated in terms of individuals or classes, typical ambiguity usually applies in any of Russell's type theories.

Propositional calculus

$\sim$ not	$\vdash$ assertion
$\vee$ or	. and
$\supset$ if . . . then	$\equiv$ if and only if

$p, q, r, \ldots$ propositions

$(p) \,.\, p$ universal propositional quantification

Predicate calculus

$(x) \,.\, (\ldots x \ldots)$ $x \,.\, (\ldots x \ldots)$	universal quantification with respect to x
$(\exists x) \,.\, (\ldots x \ldots)$	existential quantification with respect to x
$a, b, c, \ldots$	(individual) constants
$x, y, z, \ldots$	(individual) variables (free and bound)
$\phi\hat{x}, \psi\hat{x}, \ldots$	propositional functions of one variable
$\phi(\hat{x}, \hat{y}), \ldots$	propositional functions of two variables
$\phi!\hat{x}, \psi!\hat{y}, \ldots$	predicative functions of one variable

$\left.\begin{matrix}\cdot\\:\\:.\end{matrix}\right\}$	Peano dots, to reduce the use of brackets. [The larger the number of dots, the greater the scope of the corresponding brackets.]
$\left.\begin{matrix}(\phi) \,.\, (\ldots\phi\ldots)\\ \phi \,.\, (\ldots\phi\ldots)\end{matrix}\right\}$	universal quantification with respect to ϕ
$(\exists\phi) \,.\, (\ldots\phi\ldots)$	existential quantification with respect to ϕ
$=$	identity

Descriptions

$\imath$	' the ' (singular sense)
$\ni$	such that
$(\imath x)(\phi x)$	the x such that ϕx
E!	" existence " of the described individual

Definitions and assumptions

$=\ldots$ Df	equality by definition
Pp	primitive proposition

Calculus of classes

$\cup$	union	$\cap$	intersection
$\subset$	improper inclusion	$\underset{\text{prop}}{\subset}$	proper inclusion
$=$	equality	$\neq$	inequality
$\left.\begin{matrix}\sim\\-\end{matrix}\right\}$	difference (or complement)	ϵ	membership
Λ	empty class	V	universal class

Class abstraction and existence

$\left.\begin{matrix}\hat{x}\phi x\\ \hat{x}(\phi x)\\ \hat{x}(\phi x)\end{matrix}\right\}$	class abstraction, or ' the ' (plural sense)
$\left.\begin{matrix}\exists!\\ \exists\\ \exists'\end{matrix}\right\}$	" existence " (non-emptiness) of a class
E	existence of a class as in class abstraction
$\exists$	existence of a class as in class quantification

Special classes

Cls	(class of) classes
Cls^n	(class of) classes of . . . [$n-1$ times] . . . classes
$\left.\begin{matrix}\text{Cls}^\omega\\ \text{Cls}^\infty\end{matrix}\right\}$	(class of) classes of . . . classes
Cls fin	(class of) finite classes
Cls infin	(class of) infinite classes

Cls^2 excl	(class of) classes of mutually exclusive classes
Cls ex^{2}excl	(class of) classes of non-empty mutually exclusive classes
$\breve{\cup}`k$	sum of the class of classes k [corresponds to the union of classes]
$\breve{\cap}`k$	product of the class of classes k [corresponds to the intersection of classes]
$X`k$, Mult$`k$, $k^{\times}$	multiplicative class of k
Cls$`\alpha$	power-class of α
Cls consist	(class of) " consistent " classes
$\aleph_0$	(class of) denumerable classes
$\iota`x$, $\imath x$	unit class of x

Calculus of relations

$\cup$	union	$\cap$	intersection
$\subseteq$	improper inclusion	$\mid$	product
$\dot{-}$	difference		
$\dot{\Lambda}$	empty relation	$\dot{V}$	universal relation

Relations and their fields

$R, S, T, \ldots$	relations	C$`R$	field of R
$\breve{R}$, Cnv$`R$	converse of R	D$`R$	domain of R
		$\breve{\mathrm{D}}`R$, $\breve{\mathrm{C}}`R$	converse domain of R

$R``\alpha$ class of relata of members of α under R

$\dot{\exists}!$ " existence " (non-emptiness) of a relation

Special relations

1'	identity relation
0'	diversity relation
$x \downarrow y$	ordinal couple of x and y
sim	similarity [one–one correspondence between classes]
Rel	(class of) relations
$1 \rightarrow 1$	(class of) one–one relations
$1 \rightarrow \mathrm{Nc}$, $1 \rightarrow \mathrm{Cls}$	(class of) one–many relations
$\mathrm{Nc} \rightarrow 1$	(class of) many–one relations
$\mathrm{Nc} \rightarrow \mathrm{Nc}$	(class of) many–many relations

Cardinal and rational numbers

Nc, NC	(class of) cardinals

Nc fin	(class of) finite cardinals
Nc'α	cardinality of the class α
$\aleph_0$, α_0	aleph-zero
$\aleph_n$	aleph-n
η	(class of) rationals

Ordinal numbers

No, NO	(class of) ordinals
No fin, ω	(class of) finite ordinals
W	class of all ordinals
No($\aleph_0$)	second number-class of transfinite ordinals
ω	omega [smallest member of No($\aleph_0$)]
ω_n	omega-n

Order-types

ω	of denumerable series (progressions)
$^*\omega$	inverse of ω
θ, $2^{\aleph_0}$	of the continuum
η	of rational series
Ω	of well-ordered series

Index of the documents used

In this index is listed the precise location of each surviving letter of the Russell–Jourdain correspondence and of the other documents cited or quoted, and the places (if any) of their quotation in the edition. For materials in the Russell Archives no further precision of location is required. But the organisation of the Jourdain notebooks is very chaotic, and so I have given the notebooks the designation of ' I ' and ' II ' and located each document by the page number of the appropriate notebook. For Jourdain's drafts or summary drafts, only the number of the first page has been indicated.

The index takes the form of two lists. The first and principal one describes the letters between Russell and Jourdain in six columns. The first column is simply a numbering of the letters, and follows a chronological ordering which is established by the date given in the second column. The third column indicates by ' R ' or ' J ' the author of each document. Its surviving status as draft or summary draft (in Jourdain's hand, or in a copy by a relation, or both), or as a letter or carbon copy, is shown in the fourth column by ' D ' and ' L ' respectively. Its location is indicated in the fifth column either in the notebooks (when the notebook and page numbers are given), or in the Jourdain file in the Russell Archives (when an asterisk is printed). The sixth column gives the page number(s) of the extract(s) in the edition; italics indicate that the document was quoted, while roman shows that it only received mention. No indication is made of the omission of the few passages of very minor interest.

(1) *Number*	(2) *Date*	(3) *Author*	(4) *Status*	(5) *Location*	(6) *Publication*
1	16 March 1902	R	L	I, 100	*13 16*
2	17 November 1902	J	L	*	*17*
3	22 November 1902	R	L	I, 101	*17 18*
4	14 March 1903	J	L	*	*18 19*
5	17 March 1903	R	L	I, 107	*19 20*
6	10 May 1903	J	D L	I, 107 *	21 *22*
7	14 May 1903	R	L	I, 108	*22 23*
8	8 October 1903	R	L	I, 109	24
9	18 January 1904	R	L	I, 113	24
10	21 January 1904	R	L	I, 114	25
11	15 March 1904	R	L	I, 114	*24 25*
12	17 March 1904	J	D	I, 102	*25–28*
13	12 April 1904	R	L	I, 119	26 *29*

(1) *Number*	(2) *Date*	(3) *Author*	(4) *Status*	(5) *Location*	(6) *Publication*
14	14 April 1904	J	D	I, 119	30 *31*
15	13 May 1904	J	D	I, 125	30 *31*
16	15 May 1904	R	L	I, 127	*30–34*
17	21 May 1904	J	D	I, 128	*34 35*
18	4 June 1904	R	L	I, 132	*35–37*
19	14 July 1904	J	D L	I, 133 *	37–*39*
20	20 July 1904	R	L	I, 135	*40*
21	12 October 1904	R	L	I, 136	40 *41*
22	28 Nobember 1904	J	D	I, 137	*41 42*
23	20 December 1904	R	L	I, 139	*43*
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25	28 April 1905	R	L	I, 141	*43 48 49* 51
26	6 May 1905	J	D	I, 142	44 *51–53*
27	15 May 1905	R	L	I, 143	*44 51–53*
28	17 May 1905	J	D	I, 144	44 53
29	15 June 1905	R	L	I, 145	*44 53 54*
30	5 July 1905	J	D	I, 146	44 *54* 55 140 141
31	31 July 1905	R	L	I, 147	*44 54 55*
32	12 October 1905	J	D	II, 2	*56*
33	2 November 1905	R	L	II, 3	*44 56 74*
34	13 November 1905	J	D	II, 5	*57 58*
35	17 November 1905	R	L	I, 111	*58 59* 64 66
36	9 December 1905	J	D L	I, 111 *	59–*61 65*
37	15 December 1905	R	L	II, 10	*61–66*
38	28 December 1905	J	D	II, 8	*66–68 83*
39	1 January 1906	R	L	II, 13	*67–69* 83
40	10(?) January 1906	J	D	II, 11	*69*
41	13 January 1906	R	L	II, 13	*70 74*
42	16 January 1906	J	D	II, 13	*70*
43	19 January 1906	R	L	II, 15	*75 76*
44	14 March 1906	J	D	II, 17	*77 78* 80–83
45	15 March 1906	R	L	II, 22	*78–80 82–85*
46	17 March 1906	J	D	II, 24	*85*
47	21 March 1906	J	D	II, 24	84
48	22 March 1906	R	L	II, 27	*84*
49	7 April 1906	R	L[1]	II, 28	85
50	11 April 1906	R	L	II, 29	85
51	12 April 1906	J	D	II, 29	85
52	25 May 1906	J	D	II, 30	*85 86 88*
53	14 June 1906	R	L	II, 37	*88 89*

[1] This and the next letter from Russell survive in the notebooks as copies in Jourdain's handwriting.

(1) *Number*	(2) *Date*	(3) *Author*	(4) *Status*	(5) *Location*	(6) *Publication*
54	27 June 1906	J	D	II, 38	*90 91*
55	4 July 1906	R	L	II, 39	*91*
56	9 July 1906	J	D	II, 40	91
57	10 September 1906	R	L	II, 45	*91 92*
58	10 October 1906	J	D	II, 46	93 *126*
59	22 October 1906	R	L	II, 47	*93 126*
60	30 January 1907	J	D	II, 50	94
61	3 February 1907	R	L	II, 51	*95 97*
62	5 February 1907	J	D	II, 50	97
63	12 February 1907	R	L	II, 51	*97*
64	26 March 1907	J	D	II, 53	*97–99*
65	11 April 1907	R	L	II, 233	*98–100*
66	12 April 1907	J	D	II, 57	*100*
67	26 May 1907	R	L	II, 65	*100 101*
68	31 May 1907	J	D L	II, 58 *	*102–105* 108
69	1 June 1907	R	L	II, 76	*102 103 105 106*
70	8 July 1907	R	L	II, 65	108
71	19 July 1907	R	L	II, 49	*108*
72	20 July 1907	R	L	II, 49	*108*
73	18 January 1908	R	L	II, 49	*108 109*
74	15 March 1908	R	L	II, 49	*109*
75	30 March 1908	R	L	II, 75	*110 111*
76	2 July 1908	R	L	II, 76	*111*
77	19 August 1908	R	L	II, 77	111
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81	30 April 1909	R	L	II, 214	*116–118 140*
82	28 August 1909	J	D	II, 215	118
83	5 September 1909	R	L	II, 215	118 *119*
84	27 December 1909	J	D	II, 216	119 *124*
85	2 January 1910	R	L	II, 221	*119–125* 150
86	3 January 1910	J	D	II, 223	*125*
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88	9 February 1910	R	L	II, 225	*127 128*
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93	31 March 1910	J	D	II, 231	131
94	9 April 1910	J	D	II, 231	131
95	10 April 1910	R	L	II, 233	*131 132*
96	15 April 1910	R	L	II, 233	*132–134*

(1) *Number*	(2) *Date*	(3) *Author*	(4) *Status*	(5) *Location*	(6) *Publication*
97	18 November 1910	J	D	II, 234	134 135
98	22 November 1910	R	L	II, 235	*134–136*
99	24 December 1910	J	D	II, 236	136 *137*
100	2 January 1911	R	L	II, 237	*136 137* 181 182
101	26 August 1911	J	L	*	140 *141*
102	18 November 1913	J	L	*	139
103	4 September 1917	J	L	*	142
104	5 September 1917	R	L	*	*142*
105	6 September 1917	J	L	*	*142*
106	8 September 1917	J	L	*	*143*
107	11 September 1917	R	L	*	*143 144*
108	11 September 1917(?)	J	L	*	143
109	4 February 1918	J	L	*	*144*
110	11 February 1918	J	L	*	*145*
111	9 April 1918	J	L	*	*145*
112	11 March 1919	J	L	*	*146–148*
113	4 April 1919	J	L	*	*148*
114	8 May 1919	J	L	*	*149*
115	24 May 1919	J	L	*	*149 150*
116	27 May 1919	J	L	*	*150*

The second list describes other letters and documents which are germane to the correspondence. All of those from the Jourdain notebooks, whether quoted or only mentioned, have been listed ; but only quoted material from the Russell Archives is given here, since the other documents can be located by K. Blackwell and his expert staff from the descriptions given in the text. The same method of location in the notebooks or in the Russell Archives is used, with the exception of item 125, a letter which is in the possession of the recipient.

(1) *Number*	(2) *Date*	(3) *Nature*	(4) *Location*	(5) *Publication*
117	9 November 1904	Copy of letter from Russell to L. Hobhouse	II, 67	125 *126*
118	28 April 1905	Russell's notes on a paper by Jourdain	I, 117	*44–48*
119	8 March 1908	Jourdain's notes on set theory	II, 72–75	109
120	22 October 1908	Jourdain's notes on the multiplicative axiom	II, 68–70	135
121	20 April 1909	Jourdain's notes on a conversation with Russell	II, 205–209	*112–115*

(1) *Number*	(2) *Date*	(3) *Nature*	(4) *Location*	(5) *Publication*
122	11 September 1919	Letter from Mrs. Jourdain to Russell	*	*151*
123	24 September 1919	Letter from Jourdain to Miss Wrinch	*	*152*
124	26 September 1919	Letter from Mrs. Jourdain to Russell	*	*153*
125	22 November 1969	Letter from J. E. Littlewood to J. M. Rollett		*152*
126	Undated	Jourdain's poem	*	*141*
127	Various :	Jourdain's correspondence with F. Bernstein	I, 93–99	53
128	Various :	Jourdain's correspondence with Cantor	*passim*	3 6 7 27 28 36 48 51 56 Plate 3
129	Various :	Jourdain's correspondence with Frege	I, 85–89, 240	7 Plate 2
130	Various :	Jourdain's correspondence with Hardy	*passim*	7 149
131	Various :	Jourdain's correspondence with König	I, 234–237	7 54
132	Various :	Jourdain's correspondence with Peano	I, 69–71	7 76 132
133	Various :	Jourdain's correspondence with Zermelo	I, 212, 248	7 110
134	Various :	typescripts from Lady Welby to Jourdain	II, 66–67	126
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Index of subjects

Since the collection of topics covered by this book is so multiply connected, it seemed desirable to give some structure to this index by choosing a small number of main headings under which a substantial number of sub-entries could be made. The chief among these headings are 'Axiom(s)', 'Cardinal(s)', 'Class(es)', 'Definition', 'Logic', 'Numbers', 'Ordinal(s)', 'Paradox(es)', 'Proposition(s)', 'Propositional function(s)', 'Quantification', 'Relation(s)' and 'Series'. Some of these (and other) headings refer either to the singular or the plural, and each sub-entry is to be read with the appropriate choice from the pair.

'Cardinal(s)', 'Irrationals' and 'Ordinal(s)' accrued sufficient sub-entry structure to attain the status as main headings rather than serve as sub-entries of 'Numbers', which itself caters for other types of number (rational, real and so on) and also cites places where the text covers both cardinals and ordinals indifferently. Russell and Jourdain refer to certain kinds of class sometimes as 'classes' and sometimes as 'series' (which are ordered classes in their terminology), but I have made a sub-entry for each of these classes *only* under the heading with which it is mostly associated. The use of double quotation marks in "consistent", "inconsistent", "existence" and "type" has been applied throughout the index, so that Jourdain's and Russell's references to these senses are so indexed even though they did not use quotation marks themselves.

Subjects, whether falling under main headings or not, always take priority over persons. Thus, for example, Russell's theory of irrationals comes under 'Irrationals, Russell's theory of' and not under 'Russell'. The sub-entries under 'Cantor', 'Jourdain' and 'Russell' only locate (non-incidental) mention of some of their publications, and/or features of their lives and careers which are worth a special listing and cannot be placed elsewhere. I have not given any cross-references from the entries for Russell and Jourdain, as they are so numerous. In addition, the names of institutions, journals, cities and countries have been omitted when they are of only incidental interest.

A large number of cross-references are provided, in order to clarify the layout of the index. '*See*' directs *all* citations to another place; it takes the form '*See under*' when the recommendation is to the appropriately worded sub-entry of the main heading(s) then cited, and '*See above*' or '*See below*'

when directing attention from one sub-entry to another under the same heading. When *additional* entries to closely related topics are being recommended, then '*also*' is added to these instructions. I do not indicate whether references are to the correspondence or to my commentaries, or whether they are to the main text or to footnotes.

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